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# THE AUSTRALASIAN JOURNAL OF PSYCHOLOGY AND PHILOSOPHY

VOL. XXIII.

DECEMBER, 1945.

Nos. 1-3.

## INSTINCT IN THE EXPLANATION OF BEHAVIOUR.

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## I. McDougall's Position Examined.

### (1) INTRODUCTION.

While there has been a very definite trend among the writings of American psychologists to dispense with the hypothesis of instinct as a basis for human motivation, the most recent symposium, "Is the Doctrine of Instincts Dead?",<sup>1</sup> demonstrated that there are still conflicting opinions and therefore that further examination of the hypothesis of instinct and the problems it was intended to solve is justified.

Of the various systems designed to explain human behaviour, none has had such a recent vogue as the theory of instinct which is chiefly associated with the writings of William McDougall and in particular, with his "An Introduction to Social Psychology". This work was first published in 1908 and in the twenty-eight years to 1936, appeared in twenty-three editions. In the preface to the latest edition, McDougall claims that the evaluation of his work as expressed in the sales was in agreement with his own, that the work was indeed his best and that (p. xxii) "The Psycho-analysts, the Gestaltists, the Behaviourists, the Connectivists, the Characterologists, the Social Psychologists of America, the cautious middle-of-the-road men, all of these have moved further towards the acceptance of principles first clearly propounded in the first edition of this book. But we are still far from a general agreement. For myself, I am more than ever convinced that these principles are valid, and that, after the lapse of some few years, when my name shall have been entirely forgotten, these principles will be generally accepted as main pillars of a psychology which will serve as the indispensable basis of all the social sciences."

McDougall died in 1938, but there is every indication that the influence of his writings has already extended beyond the literature of pure or academic psychology to the applied branches of the subject. Social Psychology, Comparative Psychology, Educational Psychology and Philosophy, Abnor-

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<sup>1</sup> *Brit. J. Ed. Psych.*, 1941, Vol. XI, Part iii, to 1943, Vol. XIII, Part iii.

mal Psychology, Delinquency and Criminology are but a few of the more specialised fields in which McDougall's treatment of instinct has been applied to supply the basic reasons or motives for behaviour.

Such a wide diffusion of influence may be in part due to the consistency with which the author maintained the position developed in the first edition. In McDougall's own words (preface to 20th edition). . . "changes have not been of any radical kind; they have rather been of a nature to supplement, consolidate and define more clearly the views expounded in the first edition". There are, in fact, long passages of the text which remain unchanged throughout the several editions.<sup>2</sup> It would thus appear that there is reasonable justification for regarding the 23rd edition as a fair presentation of all that is fundamental to McDougall's position in regard to the theory of instinct. Unless otherwise indicated, this edition is the one referred to in the ensuing analysis.

## (2) A BRIEF OUTLINE OF MCDUGALL'S POSITION.

The basic assumption of McDougall's system is that of *hormé* or the urge to live, which attributes purposive qualities to any living tissue and thus definitely reflects his vitalistic approach. As he writes (p. 462), . . . "the hormic theory holds that where there is life there is mind; and that, if there has been continuity of evolution, of the organic from the inorganic, there must have been something of mind, some trace of mental nature and activity in the inorganic from which such emergence took place". This assumption so permeates his work that he could write (p. 447), "While the academic psychologies of the recent past have sought to explain the higher types of activity from below upward, taking simple physical and chemical units as their starting-point, hormic psychology begins by accepting the higher activities, those which are clearly and explicitly purposive and into the nature of which we have most insight, and seeks to extend such insight downwards to the simple more obscure types of action."

<sup>2</sup> The Second Edition (1909) is the earliest I have been able to procure in Sydney.



With a basic form of energy so adaptable or versatile in manifestation as to resemble the operation of intelligence, there would appear to be scant necessity to postulate the existence of a number of specific patterns of behaviour or channels through which this energy is manifested. Yet such an assumption is made. The distinct patterns are the so-called instincts, defined (p. 25) as "an inherited or innate psychophysical disposition which determines its possessor to perceive, and pay attention to, objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or, at least, to experience an impulse to such action". Another definition concludes ("An Outline of Psychology", p. 110)—"an impulse to action which find(s) expression in a specific mode of behaviour in relation to that object".

As indicated in the definition and later developed in the text, the instinctive process consists of three phases, viz., cognitive, affective and conative, and each is relatively specific. The organism is said to "perceive or pay attention to objects of a certain class"—and presumably to ignore others. The emotional excitement is of a "particular quality" and the organism acts in a particular manner or exhibits a "specific mode of behaviour" in relation to the object. There are a number of such specific innate tendencies, classified into major or primary, and minor groups. Each of the major instincts in the words of the text (p. 40) "conditions some one kind of emotional excitement whose quality is peculiar or specific to it". The seven major instincts and their accompanying primary emotions<sup>3</sup> are respectively, Escape—Fear, Repulsion—Disgust, Curiosity—Wonder, Pugnacity—Anger, Self-Assertion—Elation or Positive Self-Feeling, Self-Abasement—Subjection, Parental—Tender Emotion. Then there are "some other instincts of less well-defined emotional tendency". These include: Gregariousness, Acquisition, Construction, and,

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<sup>3</sup> The distinguishing criteria of the primary emotions are: (1) A similar emotion and impulse are clearly displayed in the activities of higher animals, and (2) exaggeration during pathological conditions.

strangely, Reproduction. The Instinct of Laughter is added in a supplementary chapter (see p. 387).

There are, too, the Non-Specific Innate Tendencies which are not classified as instincts on the ground that the emotional excitement which is specific in the case of the instincts is with them (p. 77) "rather of a many sided and general nature". These tendencies are Sympathy or the sympathetic induction of emotions, Suggestibility, Imitation and Play.

Finally, the innate endowment is claimed to include (p. 75) "some minor instincts, such as those that prompt to crawling and walking" and (p. 397) "some minor instincts . . . some minor reaction-tendencies which seem to occupy a position between the reflexes on the one hand and the instinctive responses on the other. The chief of them are, the tendency to scratch an itching spot, coughing, sneezing, yawning, urination and defecation".

Other important concepts in McDougall's system are fused or blended emotions, Sentiments and the Self-Regarding Sentiment. The compound or blended emotions are described (p. 364) as being "essentially compounds or blends of the primary emotions, that is to say, emotional qualities which are experienced when two or more of the great instinctive tendencies are simultaneously excited". Examples are Admiration, which is a fusion of Wonder and Negative Self-Feeling, Awe, a fusion of Fear and Admiration, Scorn, a fusion of Disgust and Anger, and Reproach which is a blending of Anger and Tender Emotion. A sentiment is defined (p. 105) as "an organised system of emotional tendencies centred about some object", but in a supplementary chapter (p. 437), the definition is modified to read, ". . . a system in which a cognitive disposition is linked with one or more emotional or affective conative dispositions to form a structural unit that functions as one whole system". In this way, the native propensities are influenced by the environment. The Self-Regarding Sentiment is an outgrowth or integration of the more specific sentiments and would appear to correspond to



the common notion of conscience, since judgments of values and self-knowledge are characteristic of it.

The system thus provides for the influence of the forces of nurture or the environment upon those of nature. It is architectural in its simplicity and thus lends itself readily to diagrammatic presentation. If the initial and implied assumptions are granted, the system is extraordinarily adaptable. If a man marries and reproduces his kind, he may be said to have manifested the Instinct of Reproduction. If he does not, he may be described as poorly endowed with this instinct, or he may be sublimating these energies. If a person runs from danger, he may be said to do so because of the compelling force of the Instinct of Escape and the accompanying emotion of Fear. But if a sense of duty associated with or arising out of the Self-Regarding Sentiment calls for resolute action, the person may remain to confront the danger and so move and behave generally in a manner opposed to that prescribed by crude instinct. If a lowly organism varies its customary mode of response to meet changing or unusual conditions and thus appears to act in a manner not prescribed by the specific pattern of the appropriate instinct, it may be said to manifest rudimentary intelligence, for with McDougall, instinct, which can be so automatic and efficient, "is everywhere shot through with intelligence". By assumption, McDougall thus has it both ways, immediate efficiency of action arising from the innate pattern, or adaptability. A reason may be found for every contingency, The impression does arise, however, that McDougall by assumption has endowed human and animal nature with hypothetical, instigating forces corresponding to all the forms of behaviour likely to be manifested by it and the abilities necessary to render these forces effective. His assumptions are disturbingly numerous, but he thus has at the outset everything necessary to safeguard his position and defends it with a linguistically forceful style of writing.

### (3) EXAMINATION OF SOME CONCEPTS IN McDUGALL'S ANALYSIS.

(a) *The Basic Assumption of Hormé.* This concept is consistent with McDougall's essentially vitalistic approach.

Other reputable workers have resorted to broadly similar concepts. Bergson employs the term *élan vital* to describe the apparently purposive qualities which provide the impetus and direction in evolutionary development. For Driesch, the term *entelechy* is applied to those vital properties of living tissue which are at present inexplicable or unanalysable. If such terms are used as descriptive terms expressing the author's admission of ignorance or present inability to explain the mystifying features at the forefront of knowledge in his subject, there can be no real objection to their use. But if such concepts are invoked and used as the basis of all explanations in the nature of a working hypothesis, then clearly they can be used to give reasons for the occurrence of any phenomenon in, or associated with, animal tissue and the net result may be the inhibition of enquiry rather than the pursuit of it.

(b) *Specificity and Neurological Correlates.* The specificity of the instincts has already been mentioned. On p. 29 of the 2nd edition (1909) and p. 25 of the 23rd edition (1936) appears a definite claim that each innate psycho-physical disposition depends upon some definite and presumably specific element of neural organisation. "Nevertheless," writes McDougall, "just as a reflex action implies the presence in the nervous system of the reflex nervous arc, so the instinctive action also implies some enduring nervous basis whose organisation is inherited, an innate or inherited psycho-physical disposition, which, anatomically regarded, probably has the form of a compound system of sensori-motor arcs." He then goes on to give the familiar definition which implies specificity of the cognitive, affective and conative phases, so that such behaviour as the apparently innate apprehension of danger or the mutual attractiveness of the sexes would be associated with distinct neural patterns or organisations. McDougall does refer (p. 33) to the acquisition of "new perceptual inlets" to the instinctive dispositions and has stressed the modification of the cognitive and conative phases of the process by learning and the many influences from the environment. Apparently then, instinctive behaviour must rest upon a neural correlate



which, in the cognitive and conative phases at least, becomes increasingly less specific at higher levels of the evolutionary scale. A more definite pattern in neural tissue could perhaps be expected at lower levels of evolutionary development, although McDougall has never indicated precisely the stage at which the particular neural patterns could be observed or inferred in their purest form.

Indeed the neurological and general structural basis for the hypothetical instincts is an issue involving many difficulties. There is always the general difficulty of the degree of correspondence between activity in neural tissue and experience. Behaviour which is often described as instinctive is observed in organisms which have the most rudimentary nervous systems and adaptive behaviour is possible among very lowly organisms. The particular elements of organisation in neural tissue corresponding to each phase of any one of McDougall's hypothetical instincts have not yet been isolated and the suggestion does arise that the particular pattern or inherited organisation corresponding to the instincts of Self-Assertion and Self-Abasement would be very difficult to isolate. The researches of Perkins<sup>(35)</sup> and Bartley<sup>(5)</sup> which demonstrated that general activity was present in the cortex of the dog during the presentation of a variety of stimuli and the studies of Lashley<sup>(28)</sup> and Beach<sup>(6)</sup> which have demonstrated that losses in function by rats tend to be roughly proportional to the total amount of cortical tissue removed, would indicate that if elements of organisation or pre-disposition do exist in this form of neural tissue, then older hypotheses in terms of a series of specific neurones and synapses are probably not adequate.

It thus appears that McDougall's justification for postulating the hypothetical patterns of action or instincts could not have rested upon any established neurological findings but was probably based upon observation and/or introspection.

(c) *The Justification for the Postulation of Separate Tendencies.* The procedure of endowing human and animal



nature with a number of hypothetical tendencies to correspond with the behaviour observed is not new in the history of the study of human and animal nature. It can go on *ad nauseam* until the organism is endowed by hypotheses with as many tendencies as there are observed modes of behaviour, and circular statements are made such as that an animal acquires things because it is or can be acquisitive or has an instinct of acquisition, or that the same animal is self-abasing on some occasions and self-assertive on others because it has the innate tendencies of Self-Abasement and Self-Assertion. With human beings, observation of behaviour may give a relatively poor or misleading account of the instigating forces in behaviour. Granting the existence of McDougall's instincts, a student who goes hiking could conceivably do so for reasons which could arise directly from one of the tendencies or instincts of Self-Assertion, Disgust (with his customary mode of life), Escape, or as an outlet or sublimation for Sex or Pugnacity, and the Non-Specific Innate Tendencies of Imitation and Suggestion could also be invoked. But something more than observation of behaviour would be required before the particular factors responsible for the behaviour could be determined. Even statistical data showing that significantly large numbers of people go hiking are not necessarily a justification for postulating a separate innate tendency, since people may go hiking for a variety of reasons.

McDougall was apparently aware of difficulties arising out of the postulation of a number of separate instincts and correlated neural patterns, for he writes (p. 32, 2nd ed., and p. 27, 23rd ed.)—"Owing to the complexity of the ideas which can bring human instincts into play, it frequently happens that several instincts are simultaneously excited; when the several processes blend with various degrees of intimacy." He has also stressed the widening of the cognitive and conative phases at higher levels of evolutionary development. But the problem of the blending of a number of separate instincts or patterns of behaviour which are by definition qualitatively different, still remains. The assumption that "instinct is

everywhere shot through with intelligence" would envisage some general powers of appraisal in the cognitive phase which could there bring about a reconciliation of any specific and predisposing elements. On McDougall's assumptions, this would be less difficult at higher levels of evolutionary development. But unless complete reconciliation of the predisposing elements in the cognitive phase occurred (and this would be more prolonged than most actions commonly regarded as instinctive), the problem of the blending of the different emotions would also arise. McDougall has suggested that while the cognitive and conative phases are very modifiable, the affective core of the instinctive process tends to remain basically the same at different levels of evolutionary development. A human being may be incited to anger by a greater variety of situations than a dog and manifest that emotional state in a greater variety of ways, but the emotional disturbance is represented as being essentially the same in each case. If McDougall's assumptions are valid then, it might be possible to find some support for the existence of a number of separate instincts on the basis of the differentiation afforded in the emotional or affective phase.

Once again, the general issue of the extent to which different physiological states are associated with differences in experience intrudes, but if emotional experiences described by different words could be differentiated on the basis of their chemical or physiological aspects, then the assumption of a number of separate instincts would gain support, since McDougall has implied that the separate instincts or tendencies to action are structurally differentiated. Such differentiation is difficult since the physiological basis of emotional experience is still a field for speculation. Evidence suggests that the functions of the thalamus are closely associated with emotional activity. According to Cannon's thalamic theory<sup>(13)</sup> neural organisations exist within the thalamus and these determine patterns of discharge which form the basis of different emotional experiences. The problem for research is to determine whether or not such organisations



exist as separate elements and the degree of correspondence between the number and pre-disposing influence of such organisations and McDougall's postulations.

Among the processes commonly associated with emotional activity, namely visceral reactions, the evidence does not provide any basis for the degree of differentiation required by McDougall. Cannon<sup>(11)</sup> reports that the visceral changes in fear and rage are similar. Landis,<sup>(27)</sup> too, after an extensive investigation, reports no definite and systematic bases for the physiological differentiation of different emotional states. Sherrington<sup>(42)</sup> observed the behaviour of the head end of young dogs in which he had severed the spinal cord in the lower cervical region and reported (p.396), "All the evidence obtained from all the dogs went absolutely concordantly to show that in spite of the exclusion of such a huge field of vascular, visceral, cutaneous and motor reaction the emotional states of anger, delight at being caressed by a master or at the approach of a friend, fear and disgust were developed with as far as could be seen unlesened strength". Broadly similar findings are reported by Cannon, Lewis and Britton<sup>(12)</sup> who removed the sympathetic nervous system from cats. Visceral confirmation or differentiation is, of course, not required by Cannon's thalamic theory, but the available evidence would challenge the assumption that there are any innate organisations supporting emotional response to particular stimuli, although the theory would allow of such elements of organisation being acquired, or appearing later in life through maturation.

Watson and Morgan<sup>(47)</sup> have claimed that there are at, or soon after, birth three unlearned emotional responses in children. Reactions of fear were said to be evoked by a sudden loss of support or a sudden loud noise. Rage became apparent when the movements of the infant were restricted. Responses of love were claimed to be stimulated by rocking, patting or tickling the infant or by stroking the erogenous zones. But quite a number of investigators have reported no consistency of response to these stimuli. In the extensive study by Pratt,

Nelson and Sun<sup>(36)</sup> restriction of the infant's movements was not always followed by sustained activity. In fifty-eight per cent. of the restrictions, the babies remained passive and many of them went to sleep. The absence of any clearly differentiated and consistent response to loud noises, sudden dropping and restraint have been established by J. H. Taylor<sup>(45)</sup> who largely duplicated the earlier experiments of Watson and Morgan. Irwin<sup>(23) (24)</sup> found only general undifferentiated activity in response to a sudden loud noise or falling. Sherman<sup>(40) (41)</sup> has further shown that the classification of infants' emotional responses is very uncertain, observers being influenced by their knowledge of the stimulus or situation.

Clearly, the postulation of distinct emotional states to accompany each of the several instincts is beset with difficulties. So much depends upon the cognitive phase or appraisal of the situation by the animal. Emotional experiences can succeed each other in rapid succession, consequent upon changes in the meaning or significance of the situation. So rapid is the change that the possibility of qualitative differentiation and reinforcement arising from the relatively slow processes of the viscera is questionable. As indicated previously, the qualitative differentiation associated with the patterns of discharge from the thalamus, which are postulated by Cannon, is still a matter for research. The problem would include the determination of the extent to which such patterns are responsible for different feelings or emotional experiences and the extent to which the patterns of discharge are associated with the release of specific forms of energy. It is possible that, viewed chemically, there is little or no qualitative differentiation associated with different emotional experiences, there being merely quantitative variations which acquire qualitative significance in experience according to the appraisal or cognition of the situation.<sup>4</sup>

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<sup>4</sup> Bard<sup>(4)</sup> (p. 296) points to the apparent expressions of rage and fear in decorticate cats, claiming that "In view of the available facts, it can scarcely be denied that emotions, as patterns of response, do exist. These facts dispose of the contention (Harlow and Stagner, 1938) that the specificity of an emotion is dependent upon cognitive processes whose anatomical seat is the cerebral cortex". This is a differentiation of emotions on the basis of



Despite his earlier postulations, difficulties of this kind were appreciated by McDougall, for he writes ("The Energies of Men", p. 50), "Shall we assume that each of the several instincts is activated by energy liberated within its own system? Or do all the instincts of the organism draw upon a common source of energy. . . . There is something to be said in favour of both possibilities". It is perhaps relevant at this stage to mention that Burt's<sup>(10)</sup> factorial analysis of ratings derived from "a brief schedule of emotional and moral characteristics based mainly on McDougall's scheme", indicated a general factor "e" accounting for nearly half the total variance, a bi-polar factor—aggressive and inhibited emotions, which accounted for about 12%, and a further bi-polar factor—pleasurable and unpleasurable emotions, accounting for about 4% of the variance. While such a mathematical ordering of people's manifestations of emotional reactions cannot be accepted as establishing the existence of a common source of energy, Burt's results do indicate consistently that a high intensity of one form of emotional reaction is associated with an excessive, or high, manifestation in other classifications of emotional disturbance. Burt concludes tentatively (p. 290), "It seems obvious that it is impossible to conceive the mind as divided, on its emotional aspect, into a dozen or so completely discontinuous groups of tendencies, each group representing a separate and independent instinct. I may add that I doubt whether this was McDougall's own view: for purposes of exposition, he oversimplified the picture, and made the dividing

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overt responses and is quite consistent with Cannon's thalamic theory. Sensory impulses passing through the thalamus to the cortex excite patterns of discharge which can move in two possible directions, downwards to the viscera and skeletal musculature and upwards to the cortex. The cats may not have had a normal experience of the emotion but the differentiation is based upon the responses which Bard claims are distinct for rage and fear. Harlow and Stagner raise the difficulty of classification of the behaviour, but at best this differentiation accounts for only two definite emotions and McDougall postulated several. It is possible that not all vestiges of cognition are destroyed by removal of the cortex. All reactions appear heightened with the removal of the inhibitory functions of the cortex but if any differentiation of emotional responses is claimed from such experiments, the problem of appraisal or differentiation of items in the stimulating situation remains.

lines of his classification appear far sharper than they really were."

This view would occur to many of McDougall's readers who might wonder how, on the basis of a neural correlate, observation or even introspection, one could differentiate qualitatively between the basic tendencies of Pugnacity and Self-Assertion, Construction and Self-Assertion, Fear and Self-Abasement, or how Curiosity as a separate instinct with a definite neural correlate could be isolated from the other separate instincts of Escape, Sex, Gregariousness, Construction, Self-Assertion and Acquisition. The issue arises as to whether these are instincts in the sense given to the term by McDougall, or merely terms for the classification of behaviour. To suggest that an individual is endowed with an instinct of acquisition because he is observed to acquire things, is a poor justification for the existence of a distinct instigating force or tendency, innate in the species. As the study of individual cases has shown, noticeable acquisitiveness may derive from a very complex ætiology arising out of significant experiences in early life. Indeed the principal weakness of McDougall's system would appear to arise from this confusion of classifications of behaviour with separate innate and instigating forces arising within the organism.

The terms of classification tend to become the hypothetical instincts supported by further assumptions of correlated patterns in neural tissue. The foregoing analysis would suggest that the number of separate tendencies thus arbitrarily postulated is artificially large, that in seeking to penetrate a complex problem, McDougall has carried the process of isolation too far. There is also another possible aspect of misrepresentation, namely, that human and animal behaviour is reducible to and results from the operation of a number of separate and identifiable tendencies in the organism. The ensuing enquiry will therefore involve an examination of the relevant evidence to determine the existence of any innate elements of organisation which could function prescriptively to render some forms of unlearned behaviour inevitable or



more probable than others. This will involve the discussion of the concepts of innate organisations or patterns of behaviour, first, in the restricted or local sense, of reaction to a definite object or situation, and later in the wider sense of sustained and apparently purposeful behaviour over a period of time.

## II. The Concept of Separate Patterns of Behaviour— Restricted to Individual Circumstances.

### (1) SPECIFICITY OR ADAPTABILITY?

As indicated above, McDougall has provided by his assumptions for all possibilities and his readers will note scant respect for principles of parsimony, for additional assumptions are often made at convenient points. There is the assumption of the innate and specific pattern or instinct to account for the impressive efficiency of unlearned responses, but since "instinct is everywhere shot through with intelligence", variations in the pattern are to be expected and there is also modification through learning. The elements of prescription, if they do exist, are thus difficult to discern.

The evidence from the countless studies of the natural historians would indicate, so far as a generalisation is possible, that if there are any separate and predisposing elements of organisation, they are extremely fluid or adaptable. Kuo<sup>(26)</sup> has shown that environmental conditions can influence the responses of kittens to rats. Hingston's<sup>(22)</sup> observations on wasps revealed variations in response to the same situation by different individuals and by the same individual from time to time. The encyclopædic observations of Fabre<sup>(16)</sup> <sup>(17)</sup> reveal many instances of novel and successful adaptation. Forel<sup>(18)</sup> has demonstrated that different species of ants (*Formica pratensis* and *Formica sanguinea*) can in the course of a year, become adapted to form a mixed nest and fight against the intrusions of the *Formica pratensis*. But all the studies emphasising either a change in the customary mode of behaviour which is induced by the environment, or the emergence of novel modes of response, give no indication of the elements of organisation

which are responsible for the really impressive and vital aspects of animal response, such as the apparently innate apprehension or anticipation of danger in a situation encountered for the first time or efficient nesting or mating responses in animals reared in isolation. If hormé, the basic energy, is intelligent, then such phenomena are accounted for. But the separate instincts are still postulated by McDougall. When stressing the feature of adaptation, he writes (p. 415)—“The adaptability of instinctive action to the circumstances of the moment is of its very essence; and this adaptability consists in bringing into action first one, then another motor mechanism according as the circumstances of each moment require”—suggesting that if there are any pre-disposing or prescriptive elements of organisation for the behaviour of the organism, then they function primarily and most significantly in the perceptual or cognitive phase of the instinctive process.

He is even more definite (p. 24) when he writes: “Now, the psycho-physical process that issues in an instinctive action is initiated by a sense-impression which, usually, is but one of the many sense-impressions received at the same time; and the fact that this one impression plays an altogether dominant part in determining the animal's behaviour shows that its effects are peculiarly favoured, that the nervous system is peculiarly fitted to receive and to respond to just that kind of impression.”

## (2) THE POSSIBILITY OF ELEMENTS OF ORGANISATION IN THE PROCESS OF PERCEPTION.

(a) *The Argument for Pattern or Organisation by Accumulation or Selection.* The existence of pre-disposing elements of organisation which can determine that particular stimuli are reacted to in a definite way is sometimes supported by the suggestion that such prompt appraisal and skill in action results from the accumulation of acquired skills from one generation to another. This hypothesis of the passing on of acquired skills has appeared in various guises. Rignano,<sup>(87)</sup>



for example, postulates the formation of a "specific accumulation" as a result of repetition of an act by the organism. This accumulation has repercussions on the germ plasm and thus ensures the inheritance of a pre-disposition to modes of response acquired by a previous generation. W. M. Wheeler<sup>(49)</sup> claims (p. 303), that "Instinct is essentially an inherited habit", although he concedes that in so doing, he is committing "the ninth mortal sin, known as Lamarckism, which is faith in the inheritance of acquired characters and in the opinion that the function determines the organism". Psychologically, the important feature is the actual skill displayed, which may be associated with structural modifications in ways which are difficult to analyse. A particularly thorough elaboration of the "Lamarckian" explanation of arrestingly appropriate behaviours is that of Bugnion,<sup>(9)</sup> who was very impressed by the behaviour of termites in their interminable struggle to resist the depredations of the ants. He claims that this persistent threat has been responsible for the evolution of increasingly complex defences, from the heaps of saw-dust and the pellets of saw-dust and dung of the *Calotermes* and *Termopsis*, to the building with mortar formed of earth grains and saliva, of mounds of improved design (*Coptotermes* and *Termes*) and further perfection in the cultivation of fungus within the nest (*Termes*). "To sum up our observations in a few words", writes Bugnion (p. 31), "we may pass up the termite scale from the lowest to the highest and distinguish various degrees in the growth of their industries, and, consequently, in the evolution of their instincts" . . . "The various degrees observed in the utilisation of natural elements (wood-débris, earth-grains, fungi) can be explained as we see, by a small number of very simple rational and conscious acts, which proved to be useful for the conservation of the species and were accordingly transformed gradually into habits and finally into hereditary instincts." Earlier in the article (p. 26), he wrote: "If it be true that the origin of instincts was lighted by a ray of intelligence, a reasoned and conscious form of action, and that these reasoned actions, many times repeated, become

more or less automatic habits; if we assume, moreover, that every action useful for the conservation of the species tends to leave a durable impression on the brain, and accordingly become hereditary, the genesis of instinctive activity unfolds itself clearly and convincingly before our minds. In opposition to Weismann, who refused to admit the transmission of acquired characters (psychical or physical), but in agreement with Semon, Claparède, Piéron, Hachet-Souplet, Forel and, in a general way, with most modern psychologists, I unhesitatingly maintain that the hereditary transmission of newly acquired instincts is a proven and henceforth incontrovertible fact."

Experimental evidence, however, has not so far supported this claim. The brilliantly positive results reported by McDougall<sup>(30-33)</sup> of the increasing ability of successive generations of rats of a trained line to escape from an underwater tank, when guided by a difference of illumination in the passageways, have not been confirmed by the investigations of Agar<sup>(2)</sup> and Crew.<sup>(14)</sup> In any case, such a "Lamarckian" approach assumes that "a ray of intelligence" and "a reasoned and conscious form of action—many times repeated" precede the formation of the habit which eventually becomes the instinct. In other words, the capacity for appropriate responses is assumed to exist before the accumulation of any elements of organisation which would form the basis of the specific instinct. The important issue is the indication of the nature of any innate elements of prescription which could render the initial, appropriate responses possible or more probable and this the "Lamarckian" approach to instinct, as typified by Bugnion, does not do. As with McDougall, a general capacity is postulated which enables the organism to carry out all the responses and activities for which specific tendencies are also thought to exist. It is perhaps significant that McDougall admitted (p. 303; ref. 30) that he desired positive results in his "Lamarckian" experiment, since if transmission of acquired experience were established, he could have the issue both ways, namely, a general capacity capable of embracing all the activi-

ties of the organism and ensuring adaptability, and a number of specific modes of response with an almost automatic efficiency.

The attempt to support the existence of a number of separate pre-disposing tendencies by natural selection also encounters difficulties. In common with "Lamarckian" explanations, there is the observation that in natural circumstances, unlike those of the laboratory, organisms would not always, would, in some cases, seldom encounter repetition of precisely the same situation. There is the issue of whether or not survival would tend to depend more on some element of appraisal of the configurational properties of the situation than response to specific items. The organism is not necessarily a passive thing with certain features of organisation which, unless appropriate to the circumstances, bring about extinction. In many cases, as Agar<sup>(1)</sup> (p. 187) has pointed out, there is an active selection of environments by the organism. The problem is really too complex to enable observers to point to some separate features of the perceptual organisation of the organism as being chiefly responsible for actions ensuring survival. General qualities of behaviour such as endurance and vigilance are all that could be safely postulated as ensuring survival.

(b) *Evidence for Separate Predisposing Tendencies or Elements of Organisation arising from Experimental Studies.*

(i) *Organisation or "Relatedness" in the Perceptual Field.*

There is some evidence to suggest that there are general features of "relatedness" or organisation of items in the external field which are perceived or reacted to by many animals. There are a number of studies, many of them inspired by the Gestalt School, which indicate that the animals appreciate the relationship between two discs of different diameters or degrees of brightness. If trained to react to the larger or brighter of two discs, the animals, in the majority of cases, will respond to a new situation on the basis of the



relationship and not to the particular disc to which they have been trained to react.<sup>5</sup> Such general findings have been reported by Mathilde Herz<sup>(21)</sup> using check and cog-wheel patterns of different sizes with bees. Bingham<sup>(7)</sup> has reported similar results with size discrimination in chicks and Köhler<sup>(25)</sup> too, with the discrimination of light and dark grey surfaces by chickens and chimpanzees. D. O. Hebb<sup>(20)</sup> has shown that rats reared to maturity in total darkness could react to and transpose relationships of size and brightness after very little training and mentions in the same article, his finding elsewhere that rats with a total visual experience of ten minutes could distinguish figure from ground. Such studies tend to indicate that the "relatedness" of items in the visual field is a possible feature of the perception of many animals, but it is a general feature which could apply in a variety of situations and apparently would not in itself predispose the organism to any specific line of activity.

It is often difficult to determine those aspects of a situation which are most significant for the animal. In one of the experiments of Pavlov's school, for example, the dog was found to be conditioning not to the desired stimulus but to the surgeon's white coat. Evidence to date would indicate that the properties of what appears to be the principal object in the stimulating situation can be varied without any change in the activity which is described as instinctive. Borovski<sup>(8)</sup> has shown that gulls will retrieve to their nest a variety of objects, differing in specific heat, weight and texture. The objects included small pebbles, potatoes and billiard balls. The only requisite features appeared to be the rounded shape of the object and possibly texture, since mud-balls were rejected. Lashley<sup>(29)</sup> reports that terns are disturbed by a lump of mud or wax adhering to the egg, while painting the egg a variety

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<sup>5</sup> This general position is not accepted by H. C. Taylor.<sup>(44)</sup> Working with chickens and using a more positive method of training than some of the other investigators, he claims that 81.5% of the pecking responses in the crucial trials were made to the initial "positive" grey. Perhaps the more positive method of training tended to endow the original grey with greater significance, thus weakening the "relatedness" typical of normal situations.

of colours had no influence on the behaviour. A resemblance to the rounded or oval shape of the egg of limited size seemed to be the essential property of the stimulus object.

The evidence dealing with the perceptual data would indicate few, if any, precise features of organisation in the stimulating situation as being necessary to initiate the instinctive process. As would be expected, much seems to depend upon the total situation. Lashley<sup>(29)</sup> reports (p. 456) that a primiparous mother rat will retrieve a variety of objects resembling an infant, making a selection from other objects, of food and nesting materials. The temperature, odour, colour, brightness, texture and size may be varied within limits without interfering with the retrieving. In the nest, however, a stuffed skin is pulled apart and discarded. The chief difficulty in seeking the elements of organisation necessary to stimulate the instinctive process is the lack of sufficient researches which could indicate the limits of variation of the sensory aspects of the stimulating situation beyond which no reactions are produced. If this information were available, some indication of the different compositions of the complex organisation of stimuli necessary to initiate the various processes called instinctive, could be made.

(ii) *Contributions through the various Sensory Modalities.*

The problem of indicating the elements of organisation necessary for behaviour described as instinctive has been approached by a consideration of the possible contributions through the various sensory modalities. With human beings, there are some indications from the introspective and experimental evidence dealing with Synæsthesia, that experience derived from one sensory modality can influence experience derived from other modalities. The influence of variations in sound, or beats, on the flicker-effect in visual fusion has been demonstrated by von Schiller,<sup>(46)</sup> and Zeitz<sup>(50)</sup> has shown that after-images flicker during the presentation of a vibrato tone and that there is a broad concomitant variation of apparent colour and brightness of the after-image with variations in

pitch. Cutsforth<sup>(15)</sup> has indicated the possibilities of mutual influence between tactual and visual experience. He claims (p. 148), "The tactual field does not function in the seeing individual, separately from the visual. The two modalities are not and cannot be distinct for the purposes of perception."

Whether or not the experience arising within the different sensory modalities at various levels of animal development is characterised by such mutual interaction, is of course a problem for continued investigation and relevant to any assumptions of neural correlates underlying separate instincts. Experimental investigation, however, does indicate that the pattern of activity described as instinctive is not stimulated exclusively through any one sensory modality. The action called instinctive goes forward even after an extraordinary degree of mutilation or destruction of the structures underlying the sensory processes. Stone,<sup>(43)</sup> for example, has reported that the initial copulatory response can be aroused in sexually inexperienced male-rats after afferent impulses from the skin of the anterior belly wall, the inguinal region, the ventral and lateral portions of the scrotum, the vibrissæ and from the visual, olfactory and gustatory receptors had been excluded. Such findings would suggest that the structures underlying instinctive behaviour are predominantly central rather than peripheral. If so, the evidence again suggests that the instinct is not associated with a specific pattern involving definite neural pathways, but possibly with some general pattern of cerebral excitation. Removal of a considerable portion of cerebral tissue in various positions does not prevent the occurrence of instinctive behaviour, although there is a general loss of integration in the performance. In broad agreement with Lashley's<sup>(28)</sup> findings on the learning of rats after cortical lesions, Beach<sup>(6)</sup> has shown that there is a general loss in integration and efficiency which is roughly proportional to the extent of the cerebral lesion. Nests were still constructed by the mother rat, but the nest and the retrieving and care of the young showed a general decline in precision and integration.



In the light of the available evidence, it is thus difficult or impossible to indicate any features of the perceptual organisation of animals, which in the absence of learning, could function prescriptively to ensure or render more probable specific forms of behaviour consistent with the assumption of a number of separate instincts. Even granting McDougall's claim (p. 24) that the effects of a particular sense impression "are peculiarly favoured, that the nervous system is peculiarly fitted to receive and respond to just that kind of impression", we lack the data both as to the qualities of the sense impressions and as to the organisation within the nervous system which would enable us to differentiate between the several innate patterns of response or instincts. The evidence to date, however, does suggest that in respect both of organisation in the external field and of the perceptual system of the animal, there is no marked degree of precision so that the differentiation of a number of separate patterns of behaviour, if it exists at all, will be very difficult to demonstrate.

A further possibility is that such separate instincts or elements of prescription could arise from bodily conditions, that such states of disequilibrium as hunger, or the influence of certain hormones could act as prescriptive factors and thus afford a basis for the assumption of separate instincts.

### (iii) *The Prescriptive Influence of Internal Conditions.*

The evidence in this field tends to be conflicting because of the complexity of the processes involved and the need for further researches. For some forms of activity, the influence of certain hormones would appear to be specific. C. R. Moore's<sup>(34)</sup> experiment of interchanging the gonad glands of young rats was associated with a transfer of what are considered to be the usual sexual characteristics. Feminised male rats nestled the young and the masculinised females revealed typically male reactions. Weisner and Sheard<sup>(48)</sup> report that as the young rats approached the age of weaning, retrieving to the nest by the mother declined. An injection of pituitary hormone or the presentation of a new litter brought about a

resumption of retrieving. There is evidence to suggest that instinctive behaviour does not necessarily take place because of the especial sensitivity of specific organs. Ball,<sup>(8)</sup> for example, reports normal receptive behaviour in young rats from which the uterus and vagina had been removed. Weisner and Sheard,<sup>(48)</sup> too, have reported normal retrieving behaviour in rats from which the mammary glands had been removed in infancy.

Another possibility, that the various hormones can render certain sensori-motor organisations differentially sensitive, is at present a problem for research. The evidence to date, however, does suggest that in ways not at present explicit, some hormones can have a prescriptive influence on the behaviour of the organism. It is possible that in organisms in which awareness can be postulated, internal conditions, such as hunger or tensions arising from the excretion of hormones into the blood stream, could by their persistence, influence behaviour until the condition was alleviated and in this way give rise to a consistency of conduct compatible with the postulations of the separate instincts. But unless such recurrent bodily conditions render certain sensory or perceptual processes differentially sensitive, the problem of indicating how the animal apprehends or at least reacts to the relevant aspects in novel situations, still remains.

It is perhaps to be regretted that perceptions or the appreciation of relevance, not associated with some persistent bodily condition, have not been subjected to the same searching, structural analysis as the activities of nesting and mating. For example, the sudden arousal of the emotional disturbance described as fear and the subsequent action would appear to depend on a prior act of appraisal. The problem still to be solved is whether or not, even among lowly organisms, there exists some general power of intelligence which enables the animal to make an appraisal of the situation in a manner most appropriate to its own needs, or whether there are in or working through the perceptual system of the organism, certain elements of organisation which render specific forms

of appraisal and subsequent action more probable. Clearly, the problem requires careful researches at all levels of structural development so that the contribution to behaviour of any separate elements of prescription could be studied in relation to the contribution from increasing awareness.

Thus far, the only discernible elements of prescription of behaviour which could be safely postulated are first, the physical structure of the animal, which prescribes or determines the typical postures, mode of locomotion and perhaps, in lesser degree, the habitat of the organism. Secondly, there are certain recurrent, bodily conditions such as food-deficit or the influence of the sexual hormones which in animals capable of awareness, would be responsible for a certain consistency of behaviour. But unless these bodily conditions render particular perceptual processes differentially sensitive, a decision becomes necessary as to whether or not both of the foregoing types of prescription are the same as the instincts postulated by McDougall, each of which, it will be recalled, is defined (p. 25) as "An inherited or innate psycho-physical disposition which determines its possessor to perceive and to pay attention to, objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or at least, to experience an impulse to such action."

The general impression from McDougall's treatment is that the above-mentioned elements of prescription, while they might be associated with some of his hypothetical instincts, have not the essential qualities required by McDougall's conception of instinct. He writes (p. 23), "But instincts are more than innate tendencies or dispositions to certain kinds of movement. There is every reason to believe that even the most purely instinctive action is the outcome of a distinctly mental process, one which is incapable of being described in purely mechanical terms, because it is a psycho-physical process, involving psychical as well as physical changes"—and elsewhere (p. 24) he has insisted upon the selectivity of the sense impressions which can initiate the particular instinctive



process. Thus, to reiterate, unless it can be demonstrated that bodily conditions render particular aspects of perceptual processes differentially sensitive, the above-mentioned elements of prescription have not the status of instincts for McDougall. The separate instincts must continue to have purely hypothetical status and await verification, or can be related to the above elements of prescription by making two assumptions, as did McDougall: first, that certain internal states render certain perceptual processes differentially sensitive and/or secondly, that there is a general capacity of awareness and intelligence in all organisms possessing instincts, which enables the organism to effect appraisals and actions to relieve these recurring bodily conditions. With both of these assumptions, the number of separate, innate, predisposing elements would be smaller than the number postulated by McDougall.

### III. The Concept of Pattern in the Wider Sense.

The foregoing treatment was directed to the examination of evidence for the existence of separate instincts or elements of prescription in responses to individual situations. The concept of a definite, innate tendency is sometimes applied in the wider sense of a more diffuse tendency which, while it may not prescribe the mode of operation in regard to a particular stimulus or situation, does ensure a certain persistence of effort, so that ultimately a nest is constructed or a prolonged migration undertaken and completed.<sup>6</sup> Such tendencies may be associated with teleological assumptions of an external and beneficent guiding agent which may be associated with some dim awareness or apprehension by the organism of an ultimate purpose in the life cycle.

The findings from studies of some of the most impressive animal migrations are relevant here. Roule<sup>(38)</sup> reports the results of his own investigations with salmon and those of the Danish ichthyologist, Professor J. Schmidt, with the

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<sup>6</sup> McDougall does not postulate an Instinct of Migration in his list given in "An Introduction to Social Psychology", but he does postulate a "migratory propensity" as one of the innate propensities of man—see "The Energies of Men", p. 98.

European eels. Older accounts of the journey of salmon to the sea and their ultimate return to the headwaters of the freshwater stream involved such concepts as "mneme" or "unconscious memory" or "a dim ancestral memory of the sea".

Roule indicates certain factors which form the basis of a more defensible type of explanation. The salmon are spawned in the cold, higher reaches of some inland stream. In the second year of their life, they lose, as a result of maturation, a dark layer of pigment in the skin which had hitherto protected them from the debilitating influence of sunlight. In this less active condition, the fish tend to travel downstream with the current and after entering the estuary, ultimately escape from the effects of light by moving into the deeper water of the ocean, where they may remain for a few years. Roule suggests that they may not move very far from the submarine influences of the river from which they came, so that their actual return to the same stream would not require the assumption of some form of "memory" extending over the years of their sojourn in the ocean. The precise reason for their ultimate return to the estuary is not clear from Roule's study. The instigation for the return to the estuary may be a need for increasing amounts of dissolved oxygen in the water with increasing maturity. There is also the possibility that flood waters, extending far out to sea, may initiate the processes of sexual maturity which require increasing amounts of dissolved oxygen in the water. Other explanations involve the concept of changing buoyancy as the fish increases in volume.

However, Roule has established by careful observations on the Adour and its tributaries the Nive, Gave de Pau and Gave d'Oloron that the oxygen content of the water is an important factor in determining the course of the up-stream migration, the salmon appearing to prefer the colder and more highly oxygenated streams. The greatly increased activity associated with sexual maturity and the tonic action of the colder and more highly oxygenated water are thought to stimulate the need for a greater head-pressure as a counter-irritant. This

factor of rheotaxis thus contributes largely to the exacting and sustained up-stream journey which, if successful, culminates in spawning activities in the quieter upper reaches of the stream where the optimum conditions, including low temperatures and high oxygen content, exist. While such an explanation is in need of elaboration at some specific points, Roule's work does point to the possibility of a more satisfying and defensible explanation of the migration than the anthropomorphic and somewhat mystical assumptions of unconscious memory.

Schmidt's work with the European eels would support a similar form of explanation. The eels are spawned on the northern fringe of the Sargasso Sea and in the leaf-like, larval form, cross the Atlantic, taking about three years for the journey. When near the European coast, the body changes to the narrow cylindrical form known as the elver which ascends the fresh water streams and there remains for a period of four to six years for the male and a longer period for the female. Eventually, the eels come down to the sea and cross the Atlantic at great depth to spawn on the northern fringe of the Sargasso Sea at a depth of 110-160 fathoms and at a temperature of about 20°C. Roule has indicated the following factors as contributing to the cycle. The larval form is carried eastwards across the Atlantic by the Gulf Stream. The change to the thread-like elver may be induced by the fresh and more highly oxygenated water off the coast, or may result from normal maturation. The elver, however, does display a preference for fresh and more highly oxygenated water and this factor would appear to determine the up-stream migration. The attainment of sexual maturity is associated with increasing sensitivity to temperature and light. The mature eels tend to move into deeper and warmer water and cross the Atlantic at great depth, swimming always against the flow of increasingly warmer water, until the optimum conditions for spawning are obtained.

Once again, elaboration is required at some points of the cycle, but many phases of this and other animal migrations



appear to result from a predominant contribution from antecedent factors arising from the environment, or from processes of maturation or cyclical change within the organism. Rowan,<sup>(39)</sup> for example, found in his researches with Snow Birds and Crows in Alberta, that a condition of the gonads and general behaviour typical of the spring-time could be induced experimentally by increasing the amount of daily illumination. A number of crows used in the study migrated northwards during the Canadian winter. Investigation is continuously revealing a number of demonstrable antecedents<sup>7</sup> for each of the series of phases or adjustments which constitute the life-cycle of the organism. The teleological position involving the postulation of an external and beneficent guiding agent or even the apprehension of a long-range plan or purpose by the organism has been rendered increasingly less probable and in this field, as in others, is ever on the retreat.

The device of postulating animated or purposeful tendencies is not new in the history of explanation, nor is the expedient of postulating in a substance or organism, intrinsic properties or tendencies to correspond with the reactions or behaviour observed. Essentially, the process amounts to putting into the object of interest, by postulation, those things which are eventually to be taken out of it. Combustion was once explained by the postulation of a substance, phlogiston, in all combustible bodies. The postulation was still retained despite the observed increase in weight during calcination, for it was further postulated that phlogiston had negative weight and repelled gravity. The postulation was, of course, no longer defensible after Lavoisier's quantitative studies of oxidation. The demonological theory of disease which attributed the state of disease to the investiture of the body with malignant spirits or tendencies, must have lost ground after the discoveries of Pasteur, although vestiges still linger in the attitude to the less easily understood, mental abnormalities.

<sup>7</sup> See, for example, Loeser, J. A., "Animal Behaviour", Macmillan & Co., London, 1940.

Now while the main lines of the life cycle and many of the arresting features of the behaviour of organisms is prescribed by maturational or cyclical changes and environmental factors beyond the control of the organism, so that anthropomorphic assumptions of long-range planning on the part of the organism become less probable, the problems of reactions in the "restricted" or local sense are not excluded. Even though the larval eels may be carried across the Atlantic by the compelling force of the Gulf Stream, there would be individual circumstances in the lives of these and other organisms where all the problems of explanation encountered in the "restricted" sense, such as awareness, purpose, variability of response and the possibility of elements of organisation or prescription in perception, would arise. With these basic issues of awareness and purpose, there are a number of attitudes to the process of explanation.

If the processes of human perception are regarded as capable of serving as a basis for enquiry, the procedure of accounting for an event by a description of its relevant, antecedent conditions and without reference to any assumptions of purpose in the organism, or in the wider, external sense, will reveal much valuable material. Possibly because of experience of the efficacy of the human will as an initiating factor in the occurrence of phenomena, the resort to teleological postulations when understanding is incomplete, would appear to be a characteristic of the human mind. But scientific enquiry is based upon the belief that even though antecedent conditions contributing to an event may not be discernible, they nevertheless exist and will ultimately be revealed by enquiry.

The assumption of awareness and purposive action in the lowliest forms of animal life avoids the difficulty of deciding the stage in evolutionary development at which these properties enter into animal behaviour, but there is the possibility that such an assumption would involve an anthropomorphic interpretation of the behaviour of many organisms and, as indicated earlier, a readiness to make postulations of this kind may

actually inhibit the revelation of demonstrable contributions to the behaviour or event under observation. The rigorous adherence to explanations in terms of demonstrable antecedents would reveal an increasing number of contributing factors, a knowledge of which would be essential in indicating how the behaviour takes place. But until the processes of knowing or awareness and purpose or willing by the organism are more clearly understood, there would remain in explanations of the behaviour of many animals, an irreducible core which would not be penetrated by the above methods and the neglect of which would involve a denial of the possibility of awareness and purposive action at these levels. At present, this contribution to the matrix of factors which determine behaviour is covered by a necessarily vague assumption, the difficulty being the decision as to where and in what degree awareness and purpose operate.

#### IV. Conclusion.

As indicated earlier in the article, the discernment of possible basic tendencies or elements prescribing behaviour should be undertaken at different levels of structural complexity. McDougall's treatment would have benefited if his postulations had been presented for examination with regard to organisms at different stages of evolutionary development.

Many workers now prefer to explain human behaviour in terms of present organisation, e.g., by sentiments and complexes reflecting the previous experience of the subject, and much useful understanding and therapy can be achieved in this way. But the basic problems which McDougall attempted are those of the pure science. In postulating a number of separate instincts, McDougall attempted to provide an account of the basic data in animal nature upon which the influences of the environment could operate.

The foregoing treatment would indicate that if there are separate, predisposing tendencies, innate in the organism and functioning as indicated in McDougall's definition of instinct, they are fewer than the number of instincts postulated by him



and that unless recurrent, internal states render particular aspects of sensory and perceptual processes differentially sensitive, the separate tendencies depend upon in an uncertain way and indeed are obscured by the all-pervading assumption of the intelligent nature of the basic hormé or urge to live.

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# INTERNALISING THE EXTERNAL: SOME ASPECTS OF THE PSYCHOLOGICAL PROBLEM OF THE SELF.

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THE psychological problem of the self I take to be the problem of explaining a process of internalising the external by which a distinctive centre of experience and significance is developed from within the psychological field yet remains essentially of that field. By significance I mean simply that the individual's experience and behaviour is different as the self organisation is different. Two opposite difficulties complicate the problem. On the one hand concentration on the external environment, i.e., that which is open to the inspection of a second party, may lead to a position which rejects the concept of self, a psychology without a psyche; on the other hand emphasis on distinctive internal experience may lead either to accepting a "pure ego" or to an unbridgeable separation between self and not-self: a psyche without a psychology.

In an attempt to throw some light on the problem of the psychological self I propose to review briefly the relevant contributions of Stout, McDougall, Freud, and the Gestalt school.

## I. Stout.

The way in which the individual becomes *aware* of the distinction between that which he calls his self and that which is not-self is excellently described by Stout as a series of distinctions by which we effect "a clear and explicit opposition

of ourselves as feeling, willing and thinking subjects to the material world in general, inclusive of living organisms.”<sup>1</sup>

The first level of distinction, self—not self, is at the bodily level, “whose boundary is the skin”. The young baby has certain internal sensations, aches, pains, etc., which as more or less always with him, are opposed to his perceptions of what is going on around him, sight of his mother’s face, sound of her voice, etc., for the latter class of experiences is more variable, sometimes there sometimes not. Again, over certain things he has more control—e.g., he can twiddle his toes, kick his legs—than over others: e.g., the ministrations of his mother, the constraints of his cot, etc. As a form of awareness the self at this level is a perceptual self, the idea of the self is a tied and implicit idea.

With the development of free ideas, i.e., ideas which can break loose from the perceptual situation and initiate trains of ideas, the idea of the bodily self becomes extended in temporal reference. It is no longer a momentary self; the child can remember self past, can anticipate the experiences of self in the future. The development of free trains of ideas leads to yet a further distinction or opposition in the development of self awareness. The me as predominantly bodily experience becomes differentiated into an inner self as against a bodily self. Attention, feeling, willing, anticipating may be directed not to bodily experiences or activities only but to the mental processes, the ideas themselves. The child can recollect how he thought in the past, can attend to what he is thinking now, can expect what he may think in the future. The working of the reasoning process through analysis, comparison and synthesis on this material leads to the more explicitly developed concept of the self. And by a further extension to the concept of the self not merely as it is with all its shortcomings, but as it ought to be, a concept of the ethical self.

So then, through development in freedom and explicitness of ideas in terms of a series of oppositions between that which is self and that which is not self, the relatively undifferentiated

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<sup>1</sup> Stout, G. F.: *A Manual of Psychology*, 1924; p. 647.

becomes differentiated; within the psychologically significant field there evolves an awareness of self as a distinctive centre. As more than mere distinctiveness, as having content, meaning or significance, this self idea involves a further opposition, self—other selves. From this opposition arise both the motive toward self awareness and the content of such awareness. Thus, on the cognitive side, the need to adjust his thinking or the expression of his thought to that of others, if he is to understand them or they him, must occasion in the child some critical reflection on the nature of his own thought processes. Similarly the need to adjust his emotional needs to those of others must lead to some more or less explicit consideration of his own emotional life. Further, the extension of the self to a richer, more comprehensive content is an outcome of the social setting. The child's representation of himself involves not only what he has thought, thinks or may think of himself, but what he believes others think or have thought or may think of him. Thus then, in Stout's account, a consciousness of self is developed in intricate interplay with the consciousness of the not-self, by a process of making internal that which was external, making a part of the self that which was of the not-self.

The mechanism for such extension of self content beyond mere somatic sentiency, Stout in terms of the psychological climate of his time, finds in imitation. He cites Baldwin: "So the truth we now learn is this: that very many of the particular marks which I now call mine, when I think of myself, have had just this origin. I have first found them in my social environment, and by reason of my social and imitative disposition, have transferred them to myself by trying to act as if they were true of me, and so coming to find out that they are true of me. And further, all the things I hope to learn, to acquire, to become, all—if I think of them in a way to have any clear thought of my possible future—are now, before I acquire them, possible elements of my thought of others, of the social 'alter', or of what, considered generally, we may call



the 'socius'.<sup>2</sup> This is, of course, an exposition of James' text: "In its widest possible sense a man's self is the sum total of what he can call his." That there is truth in this is unquestionable. The problem remains, however, under what conditions do certain things become his? What is meant by their becoming his? In one sense James' statement merits Spearman's scorn: "What man in his senses, even a psychologist, would call himself a banking account?"<sup>3</sup> There is here a plaguy confusion of subject and object, a difficulty to be encountered in all self theories. The truth being recognised, that all the material which may form the content of a self must come from the context in which it develops—as the ripple is but a special differentiation of the pool—the real problem is not the specification of the materials but the description of the peculiar organisation into which they enter, not "what he calls his", but the differential constellation of the "hisnesses". With regard to imitation as the mechanism for making his that which was formerly not his, we have a suggestion of an initial separation of self from the not-self. Now a completely developmental theory of the self must fully account for this distinctiveness, cannot in any way assume it. To start from such an initial separation, it appears to me, is to ground the self on something ultimately mysterious. This defect which I believe is inherent in Stout's otherwise excellent description I call separatism. I shall return to the point in discussing McDougall's contribution.

## II. McDougall.

If we take the problem of the self in psychology to involve two aspects: (1) how the idea of a self develops; (2) how such an idea becomes significant as a selective operator in the individual's experience or conduct, we may say that Stout for the most part concentrates on the first, whereas McDougall's discussion provides a useful complement in his attempt to illuminate the second aspect of the problem.

<sup>2</sup> Baldwin, J. M.: *Social and Ethical Interpretations in Mental Development*, pp. 10-11 (cited Stout, G. F.: *Manual of Psychology*, p. 653).

<sup>3</sup> Spearman, C.: *Psychology Down the Ages*, 1937, Vol. I, p. 399.

McDougall's<sup>4</sup> approach is in terms of his concept of the sentiment, derived by him from Shand. A sentiment is defined as "an enduring affective attitude towards an object" (objects, of course, being taken in the widest sense, i.e., anything to which an attitude could be directed). Viewed as a continuing "set" or state of readiness to experience certain emotions or complexes of emotions with their correlative behaviours in connection with certain objects, the sentiment is McDougall's means of linking instincts to acquired ways of behaving; the sentiments are principles of organisation by which the energies required to meet this or that situation essential for the organism's survival become integrated as potentially directed towards certain objects whose range may be far beyond that of biological needs. Now, of all the objects towards which sentiments are directed, McDougall holds the most important "object" to be the idea of the self. The master sentiment in character, which term McDougall appears to use as an equivalent to personality, is the self-regarding sentiment. From a starting point of instincts as the "prime movers" of all human experience and behaviour, McDougall sets forth to show how the highest forms of volitional conduct, the activities of the ethical self, become possible through these organised systems of conative-affective-cognitive dispositions about the idea of the self. The development may be viewed as two correlative processes, the development of the self idea and the development of sentiment organisation about that idea. For the most part his discussion of the first process is sufficiently close to that of Stout. It is in regard to the second aspect that his theory, I believe, offers something of further value as indicating the dynamics that underlie the development of selfhood in the interplay of individual and community.

This is particularly so in his analysis of the development of a social self. The social self is not to be viewed merely as a self which has social relationships; it is a self which has social or personal content. It is not just an individual in his relationships with his social environment in the sense that

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<sup>4</sup> McDougall, W.: *Social Psychology*, 1936.

he is merely set off against that environment as something distinctive and separate from it. The environment is *his*; it is a part of him as he is a part of it. As to just how it becomes a part of him, I find McDougall's discussion of sympathy particularly helpful.

The root of this internalising of the external for McDougall lies in primitive passive sympathy, literally the "feeling with", "the experience of any feeling or emotion when and because we observe in others the expression of that emotion",<sup>5</sup> as animals or humans will panic at the display of panic around them without any awareness of the occasion of that panic. Allied with the gregarious propensity, the need to *be with* one's fellows, primitive passive sympathy, the fact that being with them one *feels with* them, becomes translated into active sympathy, the need not only to be with one's fellows but the need to feel with them. Thus, to the question why we seek the approval and avoid the blame of our fellows, the answer is not simply that approval is social pleasure and blame social pain. In terms of sympathy we ourselves directly approve or disapprove, in being blamed we also blame, in being approved we also approve. The social attitude is not just something by which we are affected; it is something in which we directly participate. It is not solely of others, it is a part of us.

Three points seem to be of particular interest in McDougall's analysis: (1) his emphasis on the self as developed from basic urges of the organism; (2) his use of the sentiment as a basic unit of selfhood; (3) his principle of sympathetic induction of emotion.

(1) With regard to the first point. Just as the basic needs of the organism are such as when satisfied enable it to maintain its identity in the stream of varying conditions, it would appear that self development, the maintenance of the self within its field, is to be viewed as an extension and integration of such needs on the plane of the social and personal as an extension beyond the merely biological. In

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<sup>5</sup> McDougall, W.: *Op. cit.*, p. 79.



some sense, however, unless selfhood is to be a *deus ex machina*, even the highest levels of self-development must have their roots in the biological. Despite difficulties in his theory it is a notable contribution of McDougall, as it is also in a very different way of Freud, to indicate the possibilities of self development arising within a biological content. This holds, I believe, whether we accept McDougall's picture in its entirety as regards either his nativism or his particular list of propensities.

(2) Remembering that a sentiment is "an enduring affective attitude towards an object", it is clear that it is not the object as such but the attitude towards the object that is the important material for selfhood. Objects as such are psychologically neutral. It is what they mean to us, what we have done or suffered or may do or suffer about them, that gives them their psychological significance. Herein lies a more satisfactory interpretation of James' famous passage. What a man "can call his" are not the objects but his attitude to them, what they mean to him. A man is not "what he is interested in", but he is his interests, his loves, his hates, his sentiments.

There is a further point to be made here. As a principle of organisation and articulation the sentiment has a double aspect, the enduring attitude, and the object to which the attitude is directed. To describe the self, to specify it as something more than distinctiveness, we must go beyond the self to the object or not-self. The object *qua* object is external, not of the self; the attitude *qua* attitude is internal, of the self. Yet attitude without object is meaningless. The value of the sentiment as an organising principle, whatever philosophical or logical problems be involved, lies in its dual aspect, attitude and object. The developed self is to be seen as a structuration of the first dim impulses of the organism in terms of object attachments. Thus the self as a system of attitudes, interests, loves, a system of potential behaviour, is an outcome of the interplay of stresses, internal and external. This point

will further concern us in discussing the Freudian and Gestalt position.

(3) The third point I would stress is McDougall's principle of sympathetic induction of emotion as a useful clue to the problem of how selfhood develops by internalising the external, by incorporating within the self elements from the not-self. It was noted earlier that Stout calls on imitation to explain this. His explanation of the correlative awareness of self and others is in terms of analogical inference; e.g., when I felt this way I did that; when they do that they must feel this way. Though it is not suggested that the imitation or the inference need be explicitly and absolutely conscious, nevertheless inescapably such terms suggest a starting point of a separated self which in some fashion appreciates elements as not of itself. Observations of the spontaneous so-called imitation of very young children do not suggest any separation along the lines, *he* is doing that, *I* will try it. Nor, if Charlotte Buhler's<sup>6</sup> claim that the first genuine smile of the child at about six weeks is a social contact be even approximately true, does any suggestion of analogical inference, with again its essential separation of the me and the not-me seem likely. To say the imitation or the inference is unconscious is not helpful. In so far as the terms do suggest separation it would be preferable to use other terms. The earliest form of the individual's reaction to other individuals appears rather as directly and immediately shared experience or activity with, on the part of the child, little if any suggestion of distinctive awareness of a distinction between his own experiences and the experience of others as observed by him. Just as, when one member of the pack is frightened by some threatening circumstance, his fellows directly and immediately react in the same way, because he displays emotion, not because they have any awareness of what frightened him, nor presumably by any indirect mechanism of imitation or analogical inference—so, it may be argued, we do not have from the beginning to take the emotions and attitudes of others into ourselves, we can directly share such

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<sup>6</sup> Buhler, C.: *From Birth to Maturity*, 1937.

emotions and attitudes. This is not to deny, of course, that our understanding of such emotions and attitudes is greatly increased by such processes as analogical inference. Nor is it to deny that our repertoire of accomplishments is greatly increased by deliberate imitation. What is denied is the impossible position that such processes can operate in the first beginnings of self development.

### III. Freud.

In spite of certain marked differences, there is a kinship between the views of McDougall and those of Freud. For both the problem of selfhood involves the explanation of complex psychical processes and cultural products in terms of the differentiation and organisation of primitive biological and physiological needs as directed to or constrained by environmental conditions.

Though implicit in almost all his work, it is more particularly in "Beyond the Pleasure Principle" and in "The Ego and the Id" that we come to grips with Freud's somewhat obscure and certainly metaphorical picture of the interrelationships of driving and restraining forces, in terms of three levels, id, ego, super-ego.

The id, the impersonal it, the instinctual source of energy, unconscious, unorganised, has in itself no possibility of selfhood. It is, as id, necessarily incapable of development seeing that those aspects of id process, which modified in contact with the external become the seat of consciousness, the ego, or I, are to be taken as forming a kind of insulating surface, a protective layer, as it were, to ward off stimuli whose energies might annihilate the id. The ego as the representative of the external world, as constituting the knowing self, has such unity and order as Freudian psychology allows us. "What especially marks the ego out in contradistinction to the id, is a tendency to synthesise its contents, to bring together and unify its mental processes, which is entirely absent in the id. In popular language we may say that the ego stands for reason.



and circumspection, while the id stands for the untamed passions."<sup>7</sup>

So far the description of the ego seems to have something in common with the descriptions of the organised self already indicated. On closer analysis, however, it is plain that the Freudian ego is but a poor step-sister of Stout's or McDougall's self as a development in explicitness, stability and comprehensiveness of inner reference. The Freudian ego is weak, carrying on with forces borrowed from the id its struggle to meet conflicting demands of id urge, constraint of reality, pressure of super-ego. It maintains its ground by compromise rather than compulsion, subterfuge rather than solution, deception rather than decision, and rationalisation rather than reasoning.

The demanding, repressing forces which appear as ego forces yet lacking the ego quality of consciousness, Freud explains by postulating a third level, the super-ego. If roughly the id be described as all that is primarily biological in the individual, the ego as all such modifications on this basis as are effected by relationships with environmental conditions, the super-ego becomes that differentiation of the ego effected by the pressure of the social and personal environment. Freud contends that here he has provided a basis for what others have described as the social or ethical self, but again, the contrast between his conception and that of Stout and of McDougall is marked. It is a far less conscious self than the ego, in effect more deeply rooted in the id; its socialisation is a strangely ego-centric, or rather id-centric patchwork of loves and hates; its morality is often dim prejudice as harsh and cruel to the individual as to his kind.

The mechanism by which ego and super-ego become differentiated is that of identification. This appears to be the old principle of imitation: "What we call identification, that is to say that one ego becomes like another . . . ; it imitates it and, as it were, takes it into itself."<sup>8</sup> But this imitation is to

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<sup>7</sup> Freud, S.: *New Introductory Lectures*, 1937, pp. 101-102.

<sup>8</sup> *Loc. cit.*, p. 86.

be viewed as occurring in an emotional setting: "Identification is known to psycho-analysts as the emotional expression of an emotional tie with another person".<sup>9</sup> Freud distinguishes between outward direction of the libido, object cathexis, and its inward direction, identification. Originally, as in the oral phase, identification and object cathexis, i.e., inner and outer direction of instinctual energies, are hardly to be distinguished. Later the distinction becomes apparent; in so far as a boy's father is an object cathexis he stands for what the boy would like to have, in so far as he is an identification he stands for what the boy would like to be, i.e., the distinction depends upon whether the tie attaches to the subject or to the object of the ego. In a secondary way identification may become a substitute for an abandoned object cathexis "as it were by means of introjecting the object into the ego". This may be viewed as "a kind of regression to the mechanism of the oral phase",<sup>10</sup> that is, a regression to a stage where distinction between what I want and what I am does not arise. The ego is thus termed by Freud a "precipitate of abandoned object cathexes", in other words, the growth of the ego is by that process I have called internalising the external.

On the whole, then, this attachment of primordial energies to structures differentiated as a process of internalising the external follows a somewhat similar line to that of McDougall's development by sentiment formation. In both views an original, biological energy source gives rise to various differentiated levels of the self (which have at any rate a degree of resemblance in the two theories) through the attachment of need energies to the possible objects which may satisfy those needs. McDougall's scheme of the self-regarding sentiment, as giving an internal centre for the focusing of such energies, is paralleled by the Freudian scheme of narcissism whereby object attachments become identifications. Further, the Freudian role of identification as substitute for object cathexis enabling the ego to gain some control over its needs, i.e., some

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<sup>9</sup> *Group Psychology and the Analysis of the Ego*, 1922, p. 60.

<sup>10</sup> *Loc. cit.*, p. 62.

control over id tendencies, makes possible a theory somewhat related to both Stout's and McDougall's treatment of the self as a development in internal organisation yielding us a stable centre of inner reference. It could be argued, too, that Freud in his development has pushed an instinct theory to its uttermost, where McDougall, restrained by commonsense rather than by logic, was content with a rather uneasy half-way position which philosophically as an unreconciled dualism of inner impulse and outer object leaves the nature of instinct and the very basis of his selfhood ultimately mysterious.

But it would appear to be just this extreme instinctivism that sets the contrast between the ultimate pessimism of the Freudian position and more optimistic theories of the status and functions of the self.

For Freud all psychical products or structures reduce to the varied twists and turns of id impulse as subjected to external pressures and constraints, differentiation through frustration. But these external pressures and constraints despite identification, introjection and the like are in no real sense incorporated or mastered by the self, whether ego or super-ego. Apparent development by differentiation is no more than superficial; the depth of the id is no more changed by the impact of the external than the depth of the sea is affected by the wind that blows the waves on its surface.

All striving is ultimately from the id, yet, paradoxically, all such striving has but one aim, the end of striving. In terms of the economy principle action seeks to reduce energy, to avoid disturbance, to have done. Ultimately it seeks a return to an undisturbed inorganic state; the final wish is the death wish. In effect the development of self structure as a process of differentiation and integration of basal instinctive energies is illusion, for the initial energies cannot in any real sense get beyond themselves. The most elaborate products of modification by the external are but halting places on the "long detour to death". Freud's conception of the self gives a new significance to Coleridge's



The nightmare life-in-death was she  
Who thickens man's blood with cold.

Now this fundamental pessimism as to the status and significance of the self is the logical outcome of his system, a system amazingly consistent in spite of apparent contradictions and obscurities. His whole position stands upon what I would call his thoroughgoing internalism, by which I mean the explanation of all psychic products on a basis of organic impulse in fundamental separation from external influences.

Just what this means may be better understood by a brief consideration of Freud's view of instinct. Within the limited scope of this paper this may best be done by citing four statements from "Instincts and their Vicissitudes".

"External stimuli impose upon the organism the single task of withdrawing itself from that action."<sup>11</sup> "That which does away with a need (i.e., a stimulus of instinctual origin) is 'satisfaction'."<sup>12</sup> "The aim of an instinct is in every instance *satisfaction*."<sup>13</sup> "The object of an instinct is that in or through which it can achieve its aim. It is the most variable thing about an instinct and is not originally connected with it, but becomes attached to it only in consequence of being peculiarly fitted to provide satisfaction."<sup>14</sup> From the foregoing citations it becomes clear why Freud takes the essential feature of instinct to be "a tendency innate in living matter impelling it towards the reinstatement of an earlier condition",<sup>15</sup> thus, fundamentally a principle of inertia.

Thus, what I mean by Freud's internalism becomes clear enough. The fundamental motivation of all activities and all psychic processes is satisfaction, which is release of tension through removal of disturbances. There is no intrinsic connection between these satisfactions and the external world (i.e., the world other than inner impulse) save in the necessity of removing, avoiding, or lessening external stimuli. Note

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<sup>11</sup> *Collected Papers*, 1925, Vol. IV, p. 63.

<sup>12</sup> *Loc. cit.*, p. 62.

<sup>13</sup> *Loc. cit.*, p. 65.

<sup>14</sup> *Loc. cit.*, p. 65.

<sup>15</sup> *Beyond the Pleasure Principle*, 1922, p. 44.

that the object of an instinct has no necessary connection with it; instincts are not survival mechanisms, apparently. From all this it is plain that internal impulse can in no real sense be modified, can in no real sense provide for development.

So foreign is this viewpoint to either the English hormic school with its Darwinian background or the American functionalist school with its emphasis on adjustment, that I am surprised the contrast has not more often been stressed. This peculiar quietism is to my mind much more the point at issue with the Freudian school, than the excitement occasioned by the sexual theory. In fact the extremes of his sexual theory to which I take exception on grounds other than squeamishness illustrate this intransigent internalism, the carrying of an instinct theory to its uttermost limits.

To take only one instance—Freud uses the term “polymorphous perverse” to describe the varied and as yet unintegrated objects of libido attachment in the child. One might think that the word perverse here is merely a verbal lapse seeing that these behaviours or libido attachments must be quite normal for the child as on Freudian theory they are phases through which he passes before achieving mature sexuality. But this is what Freud has to say: “The idea forced itself upon us that the disposition to perversion is the primitive and universal disposition of the human sexual impulse from which the normal sexual impulse develops in consequence of organic changes and psychic inhibitions in the course of maturation.”<sup>16</sup>

Shades of Darwin! Unless the whole tenour of the evolutionary hypothesis is to go by the board, this with all due respect to a great man, is great nonsense. An organism directed to a condition of non-survival! As Dalbiez puts it “Psycho-sexual development is no more a succession of sexopathies than physical growth is a series of morphological anomalies”.<sup>17</sup>

<sup>16</sup> *Three Contributions to the Theory of Sex*, p. 87.

<sup>17</sup> Dalbiez, R.: *Psychoanalytic Method and the Doctrine of Freud*, 1941, Vol. II, p. 224.

The astounding position which Freud has chosen to adopt can only be understood, I believe, on the grounds of his complete internalism whose marks are: (1) all psychic process explicable in terms of conditions within the individual; (2) such internal conditions fundamentally unmodifiable.

#### IV. Gestalt School.

From its viewpoint of Gestalt or configuration as "a system whose parts are dynamically related in such a way that a change in one part results in a change in other parts"<sup>18</sup> the Gestalt emphasis on the total field situation operative in any given act or experience offers, I believe, a possible envisagement of the problem of internalising the external which avoids the initial separatism noted in Stout's cognitivist approach, helps to resolve the uneasy dualism of McDougall's instinct theory, and enables us to escape the logical extreme of one pole of that dualism, Freud's reductive internalism. On a Gestalt view the self as a system of energy becomes a particular structuration within a field of forces, as the ripple or eddy of the stream is a particular dynamic pattern of the total system of forces represented by the stream, its confining banks, and such obstructions as it encounters. The ripple or eddy is not something separated from the stream as different in kind, yet it is separated as having its own identity, is not just the stream: within the total system of forces it appears as a segregated whole or gestalt.

Within the organism which may develop selfhood the drive to action, the inner impulsion—which McDougall and Freud in their concern with inner dynamics emphasise, and to a Gestalt view overemphasise—is as with Freud a state of disequilibrium of forces, a condition of tension which psychologically we call a need. But it is only from one aspect that needs are internal. The basic biological tensions, e.g., hunger, sex, require certain objects, i.e., conditions other than themselves, for their release. The external is as necessary for the maintenance of balances of psychic forces as the internal.

<sup>18</sup> Lewin, K.: *Principles of Topological Psychology*, 1936, p. 218.



Needs are meaningless except as directed to that which meets the needs. In relationship to the need the object is said in Lewin's terminology to acquire "a valence", positive or negative. "Objects are not neutral to the child but have an immediate psychological effect on his behaviour. Many things attract the child to eating, others to climbing, to grasping, to manipulation, to sucking, to raging at them, etc. These imperative environmental facts—we shall call them valences (*Aufforderungscharaktere*)—determine the direction of the behaviour . . . The kind (sign) and strength of the valence of an object or event depends directly upon the momentary conditions of the needs of the individual concerned: the valence of environmental objects and the needs of the individual are correlative":<sup>19</sup> e.g., contrast the valence of food for the hungry with that for the replete.

It might be thought that the foregoing is but a new terminology for what Freud calls "object cathexis" or what McDougall refers to as attachment of attitude to object, constituting a sentiment. It must be remembered, however, that objects and needs are not viewed by Lewin as being in two separate universes, but as correlative aspects of a total situation. Thus needs and valences are not to be understood along the lines of McDougall's definition of instinct as a "disposition to perceive and to pay attention to objects of a certain class"<sup>20</sup> etc., wherein the disposition (need) and the objects are apparently distinct from each other, so that the nature of the disposition remains in essence unspecifiable. Nor is the relationship of valence to need of the kind Freud has in mind in his conception of instinct, wherein there is no necessary connection between instinct and object. Needs arise always in the intricate interplay of conditions operative both from within and from without the individual. A need without an object would be as meaningless as an object having psychological significance, a valence, without a need. In effect, from the viewpoint of inner experience we may speak of needs, and

<sup>19</sup> Lewin, K.: *Dynamic Theory of Personality*, 1935, p. 77.

<sup>20</sup> McDougall, W.: *Social Psychology*, 1936, p. 25.

from the viewpoint of externally observed behaviours speak of valences. If we must ask what comes first, the valence or the need—a question like the old one of the hen and the egg—the answer would be, I suppose, the need. But the question is a futile one. In the centre of the eddy we feel it differentiated from the stream, but it is *in* and *of* the stream.

Lewin's conception of needs and valences does, I think, help us to a better approach to the essential problem of the self, the internalising of the external, through a process of differentiation within a field. On a particularly troublesome aspect of this relationship of inner to outer, the relationship of self to other selves, Koffka's<sup>21</sup> discussion of physiognomic characteristics has something to offer. By physiognomic characteristics he means certain, for the most part, affective or orectic qualities which appear to belong to field objects apart from the ego needs that produce them. For example, objects appear as embarrassing, threatening, tempting, soft, melancholy, awe-inspiring. The mood or the affective quality appears to be in the object, not in us. Such characteristics would appear to arise in an organisation which includes an ego but an ego under conditions of loose segregation when the degree of tensional separation is not yet so marked as to involve an explicit separation of ego emotions and object. He argues that the usually accepted theory of pathetic fallacy that such qualities are projected from the ego to the object, as involving something originally of the ego which was not of the object, makes fundamentally inexplicable the peculiar organisation of ego and object which produces such physiognomic characteristics attributed to the object. A thing may be as threatening as it is black; a landscape may be experienced as happy, melancholy, sad or awe-inspiring as directly and as immediately as it is perceived to be a particular arrangement of "valley, rock or hill". Here, again, we have a suggestion of the possibility of some original, direct and immediate participation in the emotional, personal and social

<sup>21</sup> Koffka, K.: *Principles of Gestalt Psychology*, 1936 (see especially pp. 654-661).

events around us, of which earlier we found a hint in McDougall's treatment of sympathy. In some form, I maintain, a degree of initial participation must be granted unless not only ego and object but also ego and alter are to remain irreconcilably separated. Further, the idea of physiognomic characteristics as directly perceived meets an old objection to the direct or intentional experience of the other, an objection that we can cognise another's emotion without experiencing it as ours. Originally such characteristics would arise in a situation where ego and object are so loosely separated that mine as against its or yours or theirs can hardly arise. With further segregation in a more tense because more articulated, less homogeneous, system the particular locus of the experience becomes mine as against yours or theirs, but the possibility of such polarities would be incomprehensible unless in terms of the distribution of forces within a common field.

The Gestalt view of personality, of which they take the self to be a subsystem, thus derives from the concept of the interplay of tensional systems within the segregated system of the organism ever and always in intricate give and take with environmental forces, pressures, or attractions. Within the individual, needs have their varying degree of superficiality or deep-rootedness as belonging less or more to the ego or personal system or to the environment; they have their varying degree of duration, effective segregation; they have their varying degree of separation from or interpenetration with others, relative permeability of boundaries. The personality as a whole, as a particular pattern of forces segregated within the behavioural field, appears as gestalt. It is, however, a "weak gestalt" varying with the redistribution of forces external and internal, yet having its own identity, its own characteristic shape as a segregated configuration though a loose one.

That it does so maintain itself as a fluid equilibrium of tensional forces is explained on Gestalt theory in terms of the principle of *prägnanz*—"Wertheimer's term for the most typical form an organisation can assume and towards which

every such structure tends. It is the most general law of configuration and states that all experienced fields tend to become as articulated as possible."<sup>22</sup> A structure is what it is not merely in terms of a fortuitous arrangement arising from its history. It is what it is in terms of the type of equilibrium or balance of forces which has been evolved in its self maintenance. Just as the figure 1-: is seen as E on brief exposure or is subsequently so recalled; just as the figure (1) after an interval may tend to be recalled as (2) or, perhaps, as (3); so, too, the selection from the environment of that which



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meets a need (e.g., food to the hungry) is determined by conditions of "closure", a special variant of prägnanz whereby "certain segregated but imperfect wholes (such as memories, thoughts and actions) tend towards complete or closed forms".<sup>23</sup> So also in the personality as a whole, a complicated inter-relationship of tensions subordinate to the general or dominant pattern determines the way of life, the line of "good continuance", "closure", that to which the individual will react and the way in which he will react. To Peter Bell "a primrose by the river's brim" was a "yellow primrose" "and it was nothing more"; to Wordsworth it could hold thoughts "that do often lie too deep for tears". The cavalier felt a stain on his honour more than a wound and some have felt an attack on their banking account like the loss of a limb. Said the father of Tristram Shandy, "a man of sense does not lay down his hat in coming into a room, or take it up on going out of it, but something escapes, which discovers him".

This principle of prägnanz, precision or "good continuance", that a system, within the limits of its own structure

<sup>22</sup> Hartmann, G. W.: *Gestalt Psychology*, 1935, p. 312.

<sup>23</sup> *Loc. cit.*, p. 308.



in relationship with the field forces operative upon it, will maintain as characteristic a shape as possible, as satisfactory a degree of equilibrium as may be, involves that any given change will be met with minimal rearrangement, minimal alteration of the prevailing pattern, therefore minimal redistribution of energy. It is thus akin to Freud's principle of economy, or inertia. But there is this fundamental difference: the Gestalt prägnanz principle operates within a field, it is not like the Freudian inertia reducible to a principle governing the merely inner dynamics. It is this which enables the principle to cover the possibilities of genuine development. It cannot imply a stationary or static equilibrium because of the constant interplay of changing forces; it cannot resolve to a pull-back to the ultimately inorganic, as Freud's principle does. As a principle of stabilisation it can operate at any given level only in terms of the forces then operative. It is a principle of organic growth, not a principle of inorganic reduction.

On the whole, it appears to me a particular merit of the Gestalt approach that in bridging the separation of inner and outer it gives us a firmer footing in this matter of internalising the external, the basic problem of the self. Despite Freud, it is not easy for us to conceive of the events out of which our conscious self grew as other than in some sense conscious, in some way involving a subject ego cognising an external object. It is difficult to conceive of such events as not being in some sort ego events set in opposition to certain other events, non-ego events. But this separation, I believe, makes ultimately meaningless any thorough-going attempt at a descriptive statement of the self as a centre of inner reference developing in comprehensiveness, stability and significance. It seems to me we must endeavour to conceive ego events as initially arising as a structuration within a field of forces for which some neutral term is required. Earlier arguments as to whether the self arises from organic sensation, feeling or conation pose the wrong questions, implying as they do a degree of distinctiveness, a degree of structuration, which could only occur at a

more highly developed level of self organisation. A hint of the kind of thing we should have in mind is conveyed in Koffka's illustration: "Think of yourself as basking in the sun . . . on a beach . . . completely relaxed and at peace with the world" (you hear a cry for help) . . . "At first your field was to all intents and purposes homogeneous, and you were in equilibrium with it. No action, no tension . . . even the differentiation of the ego and its environment tends to become blurred. I am a part of the landscape, the landscape is a part of me. And then when the shrill and pregnant sound pierces the stillness, everything is changed. Whereas all directions were dynamically equal before, now there is one direction that stands out, one direction into which you are pulled . . . at the same time there takes place a sharp differentiation between your ego and the voice and a high degree of tension arises within the whole field."<sup>24</sup>

It is against some such background as this that we can appreciate the value and the limitations of Stout's series of distinctions by which we effect "a clear and explicit opposition of ourselves as feeling, willing and thinking subjects to the material world in general, inclusive of living organisms". It is in terms of some such approach that we can appreciate the contributions of McDougall and Freud in indicating a dynamic for selfhood while recognising the partial nature of their schemes as a one way approach.

There is much that remains to me obscure in the Gestalt picture. There are obvious gaps and difficulties which, if not exactly glossed over, are at any rate set aside. Nevertheless, on the whole, it does lead to a degree of clarification of an old, vexed problem. It avoids a cognitivism suggested by Stout's treatment which implies a dualism. It offers us a dynamic which does not depend on ultimately mysterious entities like McDougall's instincts, nor on equilibrium maintenance reduced to a principle of inertia barren of possibilities of development as by Freud. It does, I believe, point the way to a possible descriptive treatment of the self as the essential

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<sup>24</sup> Koffka, K.: *Principles of Gestalt Psychology*, p. 43.

pattern of the individual developing in stability, comprehensiveness and significance. Though in this matter Gestalt theory yet steers a somewhat erratic and by no means completely charted course, it gives hope of passing not without turmoil and danger of shipwreck between the Scylla of a concept of selfhood based on inner experience and so cut off from the external, and the Charybdis of a behaviouristic or physicalist description of observed events which can find no place for a self. In short, I believe that in this general direction lies the possibility of a science which can give us both a psyche and a psychology.

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## CAUSATION IN SOCIAL CHANGE.

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By Q. B. GIBSON.

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I WANT in this paper to raise the problem of the causal explanation of social events. Nearly a hundred years ago, John Stuart Mill deplored the fact that the possibility of such explanation was still a matter of controversy. A century has passed, and there continue to be many who mark off the sphere of social change as outside the realm of science. The reasons given are various, but an outstanding one is the complexity of the causal factors on which any social event depends. It is this which I wish to discuss.

I should say at once that I do not intend to raise ultimate problems about the nature of causation or the principle of induction. Let us accept these as they are accepted by any natural scientist in his work of explanation or prediction. You consider an event of a certain type—let us say, the completion of a paper on method in social science. In seeking to explain such an event when it has occurred, or predict whether it will occur, you wish to name a set of characteristics of the preceding situation, such that given these an event of the type mentioned will in fact always occur. Let us assume further that such explanation or prediction is not rendered impossible by the presence of allegedly uncaused events appearing among the mental processes, say, of the person writing the paper. Though I speak here of explanation or prediction, let us, for simplicity's sake, carry on the discussion for the most part in terms of explanation.

Let us call the characteristics of the preceding situation which are sufficient for the explanation of a given type of event, the "causal factors".



Let us accept that in speaking of a social "event", we may mean either a social change, or a social state of affairs in which change fails to occur; though it is simplest to speak in terms of the occurrence of changes.

Let us be clear that explanation is never primarily of particular events, but of sorts of event, however determinately they may be characterised. Though it is often easiest to speak of explaining particular events, we should be aware that such an explanation must apply equally well to any event of this determinate sort.

Again, let us recognise that the causal factors will vary according as an event to be explained is characterised in different ways—for example, more or less determinately. Thus you would not expect the same answer to the question "What is the cause of war?" as to "What is the cause of civil war?" or "What is the cause of war between America and Japan in the twentieth century?"

Finally, let us recognise that the causal factors which can be offered in the explanation of any type of event are in two dimensions. You may look at the spread of a number of factors which are at first sight causally independent of one another; and you may also take each such factor and ask concerning its cause, so tracing origins back into the past.

Keeping all this in mind, let us consider whether there is anything about the operation of causal factors in social change which makes them especially difficult to deal with, and if so, whether there is any effective way in which this can be overcome.

First of all, it is frequently pointed out that the number of factors entering into the explanation of any social occurrence is very large, and that it greatly exceeds in number any which have to be dealt with in the "natural" sciences. Consider, for example, an event like the declaration of cessation of hostilities with Japan. The type of decision made by leaders of the Allied Governments at Potsdam, features of the economic and political systems of the major countries, the state of

scientific technique leading to the manufacture of the atomic bomb—all these and many more have to be taken into account. Factors of all types—psychological, economic, political, and so on—enter into the picture. In the midst of all this, how can we disentangle general laws of social change, and even given that we know the general laws, how can we apply them to the explanation of this special type of event?

It may be further pointed out that it is not only the largeness of the number of factors in any given case; it is that the number of different possible combinations of such factors is almost infinite, and that for each combination there will be a difference in result. This, it may be said, is why human history does not repeat itself, or, to speak more accurately, why there is a greater difference between the characteristics of social events occurring at different times and places than there is, say, between different physical and chemical events. And it is this very lack of approximate repetition, itself the result of the large number of possible combinations of factors, which makes it all the more difficult to isolate the factors in any given case by comparison with similar instances.

We must admit the truth in all this. At the same time there are two points to be noted.

Firstly, the largeness of the number of causal factors varies greatly with the determinateness of the type of event to be explained. If the event is of a very specific type—for example, that of which the declaration of war between England and Germany in 1914 is the unique instance—the number of factors is certainly very large indeed. If the event is of a very general type—say, war—the causal factors, when tracked down, will be very much fewer. The trouble in this latter case is rather in the disentangling of the factors from the large mass which are actually irrelevant for the occurrence of such a very general type of event. This is why in such explanations we tend to fall back on a plurality of causes, saying war is sometimes due to this combination of circumstances, sometimes to that. Instead of enumerating a large number of causal factors, we resort to enumerating a large number of alternative sets of

causal factors. When we do this, the difficulty re-asserts itself in another form.

Secondly, it may be pointed out that what is here said of social explanations is true to a very great extent of all explanations. Isn't there always a great background of conditions without which an occurrence of any special sort would not take place and which must therefore be taken into account as causal factors in its explanation? And if so, isn't the difficulty raised about social explanation a quite general one about all scientific explanation?

This appears to be a matter of degree. It seems that you need only compare social events with astronomical events to see a sheer difference in number of causal factors involved. It may of course be said that every breath of wind through the trees has *some* effect on the movements of planets and stars. We may go on to say with Whitehead that every event in the universe must be counted as a causal factor in the explanation of any single event. This, however, is a speculative hypothesis, and the more usual procedure is to regard only *some* of the characteristics of the preceding situation as relevant for any explanation. If we do this, there is good ground for saying that there are fewer such relevant characteristics—i.e., fewer causal factors—in astronomical, physical, chemical, even biological explanations, than in social ones.

But does this really meet the point? If this difference is only a matter of degree, and we admit that even in the physical sciences the number of causal factors to be taken into account in the complete explanation of any relatively determinate event is very considerable, is this sufficient ground on which to rest the special difficulty of social explanation? At this point, it may be suggested that the special difficulty lies not so much in the mere number of causal factors involved, but rather in something else. Admitting a comparatively large number of factors in each type of explanation, it may be said that in the physical sciences we have a simple criterion for selecting the important factors, whereas there is no such criterion in the case of the social sciences.

This brings us to the next stage of the argument.

We may say that we do not wish to deny that the number of factors to be taken into account in the complete explanation of social events is large, any more than we would deny that it is considerable in the explanations of physics. The point, we say, is that we have to discriminate between these factors. Some are obviously important, some are not. To say that we can never get anywhere because of their multiplicity is to miss this point. In physical explanations we recognise that there are masses of conditions which, while not strictly irrelevant, we can for practical purposes neglect. Why not recognise likewise in social explanations that our job is to throw the spotlight on the factors of crucial importance, and be satisfied with that? Admittedly, we say, the murder of an Archduke at Sarajevo was a causal factor leading to a declaration of war in 1914, but the important factors were, say—and then we start to argue—the aggressive attitude of the German government, itself largely the result of the Prussian military tradition, or features of the structures of the German and British economies, and so on.

This idea of varying degrees of importance among causal factors in social change is a commonplace of ordinary argument. It is applied here, it should be noted, to the causal factors in the explanation of *any given* social event. From this basis, it may be suggested, it can be quite well extended to explanations covering wider and wider fields of social events. Events are, after all, not atomic units. There are events within events, and these within other events. What we call an event depends on what characteristics of an ever-changing situation we decide to indicate. The sinking of such and such a battleship is an event, so is, say, the Battle of the Coral Sea, so is the Second World War, so is the transformation of world politics of the sort that has taken place in the twentieth century. Suppose then that we apply to large numbers of events within a given area and a given period the principle that certain crucial factors in the explanation of each are the most important. Among these, it may be said, we are able



to select certain factors, or at any rate certain general types of factor,<sup>1</sup> which are more important than others over the whole range. As we extend the range, we are led on to frame what is commonly called an "interpretation of history". We may argue with Mill that the most important type of factor in the explanation of social changes generally is the state of the knowledge and of the prevalent beliefs of mankind. Or we may argue with Marx that the most important type of factor is the state of the relationships men enter into in the course of production. But to present the issue in this way—in terms of which among a large number of types of factor is causally the most important—is to avoid on the one hand the danger of asserting that one type of factor (intellectual, say, or economic) provides the sole and complete explanation of social changes, and on the other the danger of turning away in agnostic despair before the vast multiplicity of causal factors in human history.

This is all very good. But what is meant by "importance" here, and how do we discriminate between degrees of importance among causal factors? Though we do habitually speak in terms of such degrees of importance, what precisely is meant by it is often far from clear.

"Importance" is, of course, a very general word, and one used differently in different contexts. It is often used, for example, to indicate what specially interests us, or what we conceive to be most worth while. If this were all that were meant, however, when we speak of the importance of certain factors in causal explanation, the idea would clearly be of no help to us in our problem. Furthermore, the causal use of "importance" tends to suggest something distinct from its evaluative use. A certain type of change, for example, may be important in the sense of my being excited by its occurrence or of my considering it desirable, without this making it an

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<sup>1</sup> The distinction between "factors" and "types of factor" is only one of degree. A "factor" is never a particular event or state of affairs, but always a type or characteristic, however specific, of events or states of affairs.

important factor in the scientific explanation of any subsequent event or course of events.

Confusion may arise on this point from the fact that we often speak of events as being important as means to desired or desirable ends. We here combine the causal and evaluative uses of the word. If I wish to find out how it was that I achieved my desired end of getting into power, or to work out a program for getting into power, I may say that the occurrence of some specific event was, or will be, of crucial importance. In such a case it may be called important both because it is an object of my especial interest, and because it is the important factor in the production of the event in question. But we have still to make clear how it is that we can speak of it as important in the latter sense

The first thing to consider is whether we can help ourselves out by returning to our comparison between the explanation of social and of physical events. If we can see how we discriminate between important and unimportant factors in the latter case, it may be possible to use this as a cue in examining the former.

We are quite familiar, in the explanation of physical events, with the distinction between *the cause* of an event, and the background of *conditions*, without which admittedly the event would not have taken place, but which are not as important as the cause. Sometimes this is identified with the distinction between the necessary cause on the one hand and what must be added to make this into a sufficient cause on the other. If, however, the distinction between cause and conditions is reduced to this, it will not help us at all with our problem. If by a "necessary" cause, we mean that in the absence of which the event would never occur, and by a "sufficient" cause that in the presence of which the event would always occur, then it is clear that (plurality of causes apart) every single causal factor could in its turn be counted as a necessary cause. The sufficient cause is simply the sum-total of conditions (or causal factors); each causal factor is a necessary cause. It is precisely this fact that *every* causal

factor is necessary which makes it so easy for us to say that each is of absolute importance for the occurrence of the event, and that there is no way of discriminating between them. If any factor had been different, the event as characterised would not have occurred. Discussion of necessary causes, therefore, merely forces home the problem about degrees of importance.

But is there not something further involved in the distinction between cause and conditions, to which we can appeal? Surely, it will be said, what is being indicated is the distinction between the factors which are *changes* and factors which are enduring *states of affairs*. The preceding situation has various characteristics which are relevant for the occurrence of the event, but which remain unchanged until after the event has occurred. But the preceding situation is also characterised by the occurrence of changes of a certain type, and as soon as certain of these changes have occurred, the event takes place. I wind my watch and shake it and it goes. The winding and the shaking are "the cause" of its going. We recognise that without a large number of enduring features of the internal structure of the watch, it would not go at all, but these are to be counted as background conditions, and as long as they do not change we may choose to neglect them as unimportant.

We have, then, a method of discrimination which depends on the stability of a great number of the causal factors. If the universe were in a perfectly static condition, it would remain so, and explanation of this condition would be impossible. But it departs from such a static condition and it is these departures which we rightly seize on as the crucial factors in the explanation of further departures.

The question now is whether we can apply this principle of selection in the explanation of social events. Clearly we can and do. There were features of the condition of Europe prior to 1914 without which a declaration of war would not have occurred, but which remained stable and could therefore be relegated to the background of any explanation. Such, for example, would be the bare presence of central governments

with armed forces at their disposal in Germany, France and Britain.

But the trouble is that in social explanations, this principle of selection does not take us very far. And this for the simple reason that among the relevant factors preceding any social event, the number of factors which are themselves changes is still very large. This, it may be said, is the real reason why we cannot select our crucial factors in social explanations as we can in physical ones. Just as there is very much less approximate repetition in social change than in other types, so also there are very many fewer stable characteristics in social states of affairs. If you try to think of the stable factors in any process of social change, you tend at once to turn to biological factors of heredity or the geological features of the earth's surface. Among the social factors entering into the explanation of any social event, there are relatively few which are not subject to constant modification.

The question still remains then—is there any way of selecting from among the many relevant *changes* preceding any social event those which are more important?

It may be pointed out that we can select from among the relevant preceding changes on a temporal basis. There are the ones which immediately precede the event in question, and there are ones which are more remote. Mightn't we select the more immediate causal factors as of greatest importance in the explanation of an event?

This is a great way of simplification, and one that is very frequently followed. Memories are short, and remote causes do not force themselves vividly on the imagination. Most people asked the cause of the American declaration of war on Japan in 1941 would say—the Japanese attack on Pearl Harbour. The Emperor Hirohito has explained the acceptance of surrender terms as the result of the dropping of an atomic bomb.

But some future—even if not contemporary—historian may raise the question whether these events, though admittedly



causal factors, were the really important ones. Preceding changes, which they will proceed to analyse, will be shown to have put the world into such a state that an event of quite minor importance will finally set off the event in question. We recognise that very often they will be perfectly justified in doing this sort of thing. The immediate factor may possibly in certain circumstances be the really important one, but its being immediate should be quite definitely distinguished from its being important.

How then do we distinguish the important factors? There does seem to me to be a real criterion, and it is one of which, implicitly, we make constant use. We recognise that each of the many changes which can be counted among the causal factors leading to a given type of social event is necessary for the occurrence of that type of event. In that sense, each such change is absolutely final and decisive. Without it an event of that precise sort would not have occurred. But the point is that even if many of these changes had not taken place, or had taken on a completely different character from what they actually did, an event *very closely resembling* the one in question might still have occurred. In this case we would put down these changes as unimportant causal factors. Whereas, if the absence of a factor would have led to an event *very different in character* from the one in question, we would say that it was an important causal factor. The criterion suggested here is—how great a difference in the character of the event to be explained would have been made if the factor had not been present. The causally important factors are those the absence of which would have made a radical alteration in the character of the event.

Take the question whether the Japanese attack on Pearl Harbour was an important factor leading to the declaration of war by the United States on Japan. Without it, the declaration would not have taken place in the way it did. But it may be argued that in view of other factors it would have taken place not long afterwards, with the circumstances somewhat changed. If this is true, then it was a relatively un-

important factor. On the other hand, if the development of American industry in the preceding period had not been so rapid, it may be argued that nothing like such a declaration would have taken place at any time. If this is true, the expansion of American industry was a relatively important factor.

This way of estimating degrees of importance is a fairly obvious one. All I want to do here is to make it explicit and to emphasise that it does provide a criterion for selecting important factors even in the explanation of social change, thereby enabling us to overcome to some extent the difficulty about the large number of factors that are involved. It is sometimes put by saying that the important factors are those which determine the broad character of an event, the unimportant ones those which determine the details. To distinguish between these is essential for estimating the adequacy of our explanations. Complete explanation in terms of all the relevant changes of all sorts in the preceding situation is admittedly in most cases impossible. Neglecting the factors which "do not make much difference", and arranging others in the order of how much difference they make, is the only means at our disposal.

It should of course be pointed out that this procedure becomes less necessary, the less specific is the type of event to be explained. As I indicated earlier, in dealing with very general types of event—say, war, as distinct from specific wars embarked on in specific circumstances—the causal factors, if properly tracked down, will be few. And these will all be relatively important, in that, by definition, they determine the broad character of "war", and not the details. But this does not alter the main point that in so far as you have a multiplicity of causal factors in any explanation, there is a definite principle underlying the estimation of their degrees of importance.

It may be said—how do we know that there may not be in any case a large number of factors of *equal* importance, such that, in the absence of each of them, an *equally* large difference

may be made in the character of the event? The answer is that we can never know this *a priori*. All we can say is that we have no especial reason to expect it, and very good reason for starting an enquiry as to whether there are specially important factors in each case.

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We may illustrate this criterion of causal "importance" by raising the question whether there is any connection between the *largeness* of a change and its importance as a causal factor. Are changes which are small relatively to other causal factors, or relatively to the change to be explained, or both, ever of the greater importance in the explanation, in the sense that their absence would have made the greater difference in the character of the event?

This sort of question is often raised in discussions about social change. On the one hand we tend naturally to associate importance with size and to rule out small changes as "insignificant". On the other hand it is picturesque to think of great events having small beginnings, especially when they are decisions of this or that colourful individual. We ask this sort of question: Is it possible that the discovery of a single scientist about how to split an atom of uranium might be the crucial factor leading to the destruction of civilisation? Is it possible for the large economic and political factors to be in such a state of balance that a single decision could alter the lives of large masses of mankind?

We should be clear first of all as to what is meant here by a "large" social change. The word "large" may be used to indicate one or other or both of two very different features of a change. A "large" change may mean firstly one which covers a large area or (when applied to social changes) a large number of persons. In this sense, a discovery made by one man is small compared to a general change in public opinion. But it may mean secondly a change in which the final state of affairs is very different in character from the initial state of affairs. In this sense, a reduction in the sugar ration in a

given area is a small change compared with a general strike in that area. To avoid ambiguity, it would be well not to speak of "large" changes, but on the one hand of, say, "extensive" changes, and on the other of "radical" changes.

It will be clear that you can distinguish between the extensiveness of different changes without taking into account how radical they are. The slightest of changes in the general attitude of the electorate towards the government, for example, would be more extensive than the sharpest of revolts in the Labour Caucus. On the other hand, in deciding how radical a given change is, you have to make clear to what "extent" you are referring. The death of one individual would, for example, be a radical change when you are considering only this individual: it would not be a radical change when you are considering the population of the whole country.

When the question is raised whether it is the large changes which are the important factors in explaining further social changes, we may be referring either to extensive changes or to ones which are radical relative to some given area or population. It may be said that extensive changes are to be explained principally in terms of other extensive changes, in which case you would discount the importance of such factors as the decisions of politicians, illnesses of generals, propaganda of small sects, discoveries of individual scientists, in favour of, say, general changes of feeling of the population, the equipment of armies, the agitations of mass movements, the steady development of knowledge in the scientific world. Or it may be said that radical changes are to be explained principally in terms of other radical changes, in which case you would discount the importance of, say, revulsions of political feeling on specific issues or detailed modifications of military strategy in explaining revolutions or military victories. It is, of course, quite possible to use both of these criteria of "largeness" in discussing this question, and this is what is usually done. The discovery of a scientist as to how to split an atom, for example, is neither a radical nor an extensive change,



while the "destruction of civilisation" is both. In what follows when I speak of "large" and "small" changes, I assume that we are keeping in mind that both these criteria are involved.

If it is the large changes (in the sense of those that are radical or extensive or both) which are the causally important ones, it may be suggested that this will make the work of explanation very much easier. Such changes are easily observed, and few in number, whereas "small" changes are many and easy to pass over. An argument against the possibility of social science has in fact been based on the assertion that small changes are of especial causal importance in human affairs.<sup>2</sup>

This argument is worth examining carefully. We might develop it for ourselves as follows. There is an old rhyme which tells of how a battle was lost all because of the loss of a horse-shoe nail from the general's horse. Now preceding the loss of a battle there would be thousands of such non-extensive, non-radical incidents as the loss of a horse-shoe nail, many of which would never be recorded. How, it may be asked, is it possible to select the causally important factors from among all these? In the same way, if the crucial factor in the explanation of, say, the Russian Revolution of November, 1917, could have been some single decision of an individual, think of the hundreds of thousands of such decisions which would have to be considered.

This argument, as it stands, is clearly not very convincing. Assuming that small changes may at any time be the causally important ones, we do seem to be able to discriminate between them and eliminate most of them without much trouble. We fix quite easily on the loss of the horse-shoe nail as against, say, the loss of a hat by an ordinary trooper or the development of an attack of rheumatism by the regimental cook.

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<sup>2</sup> This, for example, is how I interpret the argument of Dr. Irving Langmuir in the paper "Science, Common Sense, and Decency", given as the Retiring Presidential Address to the American Association for the Advancement of Science, and reprinted in *Nature*, March 6, 1943, in which he points to the vital importance of "divergent phenomena" in human affairs.

Likewise, we point at once to some political or military decision of, say, Lenin or Trotsky, in explanation of the Russian Revolution. The point is that *if* we know the large factors in the case and have considered their causal importance first, we can then quickly select those few small changes which are likely to be "most relevant" in the general situation. The decisions of leaders are an obvious case in point. Once you know what the broad characteristics of the changing situation are, you know at once what sort of decisions of what sort of individuals might make "all the difference". The argument, in fact, would only hold if you were starting from scratch in your explanation and taking no especial notice of the large factors. But the large factors being few and easily observed, this situation is not likely to arise.

We are still left, however, with a feeling that some special difficulty for social science is created if large events can be explained mainly in terms of small. That there is such a difficulty must, I think, be admitted, but it lies not so much in the selection of the crucial small events as in the explanation of these small events themselves. If we ask why the horse-shoe nail was lost, or why the decision was taken, we are forced to investigate detailed incidents of the blacksmith's shop, or unravel the complexity of individual motives. For the student of large events, this is not only distasteful, but usually difficult. The same point can be seen if we look at it from the point of view of prediction. Once we see the horse stumble or the decision being acted upon, it may not be so very hard to estimate its effect on the course of the battle or the revolution. The trouble is that such small events cannot themselves be so easily predicted. It would require a detailed knowledge which no one could ordinarily be expected to possess. This is the reason, it seems to me, why any social scientist would prefer to discount the importance of small changes in the explanation of the large ones.

The question still remains whether small changes can be so discounted. The question is clearly one which cannot be

decided on logical grounds. It does not follow from the fact that a change is small that it is causally unimportant, and whether it is really so can only be determined in the course of empirical investigation. All we can do here is to point to certain arguments on either side which are based on a misunderstanding of the issue. On the one hand, for example, the importance of some small change is challenged on the ground that it is only one factor among many. The decision of a leader, it is pointed out, would not have had the effect it did, apart from the favourable circumstances of the moment. This, of course, is true. There is always a large number of causal factors involved in the explanation of any event; but this still leaves open which are the more important—the decision of the leader, for example, or the favourable circumstances.

On the other hand, where it seems easy to point to a small change as being crucial in producing an extensive and radical effect, it must not be forgotten that the causally important factors must be looked for in two dimensions. If small changes lead to large, it is equally true that large ones lead to small. The important factor in the loss of the horse-shoe nail, for example, may well be the inefficiency of the cavalry's repair services, and behind that we may see the general bad organisation of the armed forces. Similarly, the scientist's discovery about splitting the uranium atom may have as its important causal factor the preceding accumulation of physical knowledge throughout the scientific world. In this way it might be argued that the larger factors would always have the predominating effect *in the long run*. This point should be kept in mind if one is to come to a fair decision on the issue.

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So far, we have discussed the problem of the selection of causally important factors for the explanation of *some given* social event. We have still to consider whether it is legitimate to extend the principle of selection to explanations covering wider and wider fields of social events. Can we point to some

general type of factor, the alteration of which would make an outstandingly large difference in the outcome, over the whole range of social events? In other words, is it reasonable to attempt a general "interpretation of history"?

We should be clear what is involved in such an extension. No one, I imagine, would hold any one type of factor to be causally the most important in the explanation of every single social event. The most important type of factor in one case, for example, might be intellectual, in another economic. Nor would it be very sensible to say merely that one type of factor is causally the most important in the explanation of *more* events than any other. "Events" are not isolable units which can be numbered off in this way. It is clearly necessary to introduce the distinction between "large" and "small" events, as we have defined it. The man who frames a general theory of history is interested in pointing out the type of factor which is more important in explaining the more extensive changes, and the more radical of the more extensive changes. In estimating which is the more important type of factor on the whole, we intend clearly to weight the different ones firstly in proportion to the "largeness" of the events into the explanation of which they enter, and secondly in proportion to the degree of importance they possess in the explanation of such events. Thus, changes in economic relationships would be considered more important than, say, changes in political structure or prevalent beliefs or in individual decisions, if it were the case that, had they been different, this would have made the greater difference in the course of the more extensive and the more radical changes in all spheres of social life.

If we make clear in this way what we mean by the general importance of types of factor, we clearly have a scale on which to discriminate between types of factor, just as we have a scale on which to discriminate between the factors entering into the explanation of any given event. As in this latter case, there is of course no way of knowing a priori that there are not types of factor which will be found to be of equal causal importance.



Furthermore, it is always possible that one will be found to be more important over one field of events—i.e., in one area or in one period—and another in another area or period. We should not, however, be prevented from attempting to discriminate by any assumption of equality of general importance, nor should we be prevented by the possibility of oscillations in general importance from investigating whether an interpretation found true for one field could be extended.

It may be said that the estimating of degrees of general importance over large fields of social events would be altogether too vast an undertaking, and one on quite a different level of difficulty from the mere selection of important factors in explaining some given event. The answer to this, I think, is that it is made possible by the weighting given to the radical and extensive changes. In practice it is through concentration on these, and the neglecting of large numbers of comparatively small events that any general historical interpretation has to be supported. Even so, the historical knowledge required is sufficiently vast to make any social scientist beware of dogmatism.

A further objection which is often raised against the making of hypotheses which assert the general importance of some type of factor is that the different types of factor are constantly interacting. It may be said that we should not neglect the second dimension of causal explanation at this point. If we point to economic changes in Germany, for example, as largely responsible for the Nazis' seizure of political power, were not these changes themselves due, among other things, to political defeat of Germany in the First World War? Changes in the relationships of production may be important in explaining changes in scientific knowledge, these in explaining changes in technique, these in explaining changes in the relationships of production—and so we could go on. It is the old story of the hen and the egg. In view of such constant interaction, can we call any type of social change more important than any other?

It appears to me that this objection does not hold. We must of course admit constant interaction between types of factor. There are no first causes in social change, and its complexity is such that causal factors of all types, both immediate and remote, are to be expected in the explanation of any given change. But this does not in the least prevent us from discriminating between different types of factor in respect of their general importance. Let us recognise, for example, that political factors will be the most important in explaining certain economic changes, and economic factors the most important in explaining certain political changes. But if we find that economic factors make a *greater* difference in the character of political changes (as always, weighting the large ones) than political ones in the character of economic changes, we would still be justified in attributing greater general importance to economic factors.

Such differences of importance will be further accentuated if changes of any given type are relatively more important in explaining further changes of that same type. Thus, if we find that economic factors are more important in explaining further *economic* changes than political ones in explaining further *political* changes, this will reinforce the greater general importance of economic factors. This is the sort of situation which is indicated when it is said that the economic system develops within itself in relative independence of political factors, whereas political institutions do not develop in relative independence of economic factors. When interpreted in terms of degrees of importance, such phrases are perfectly compatible with admission of continuous interaction.

To illustrate with a deliberately simplified example—consider the economic situation in Germany in the 'thirties, and its relation to the Nazis' seizure of political power. It may be put forward on the one hand that economic developments in Germany were important factors leading to the Nazi revolution, and on the other that Nazi political control was an important factor leading to subsequent transformations

of the Germany economy. Now suppose it were the case that without the preceding economic developments, the Nazis would not have seized power and the political history of Germany would have been radically altered, whereas without Nazi control the German economy would nevertheless have developed in its broad features sooner or later along the same general lines. We would then say that this was evidence in favour of the greater relative importance of economic factors. And this conclusion would be reinforced if we were able to point out that there were certain features of the period of economic collapse without which the Nazi economy would not have developed in anything like the way it did, whereas the political developments under the Weimar Republic had only a minor effect in determining the course of the Nazi revolution.

I have selected this type of example because the objection we are discussing has been brought forward more particularly in controversies concerning the materialist conception of history. Against this hypothesis it seemed to be peculiarly effective, firstly because Marx initiated the use of an unfortunate metaphor about the economic "foundations" and the cultural "superstructure", which suggested a denial of causal interaction between types of factor, and secondly because Engels, when he sought to correct this impression about such interaction, continued to speak of the economic factors as the "real basis" and the "most decisive", without making clear how this was to be interpreted. If this is interpreted in the way we have done, in terms of differences of causal importance, it seems that pointing to the fact of causal interaction between types of social change need not frustrate any attempt to present some "interpretation of history".

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In this paper, I have tried to show how the conception of degrees of importance may be used in resolving the difficulty concerning the complexity of causal factors in social change. I have adopted the word "importance" simply because it appears to be commonly used in the making of the sort of

discrimination which I have tried to describe. I am not suggesting that there are not other contexts in which reference to the "importance" of a social event may mean something different. But the examination of these would take us beyond the main theme of the present discussion.

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## DISCUSSIONS.

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### I. THE SUBJECT OF ETHICS.

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By ARTHUR N. PRIOR.

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PROFESSOR JOHN ANDERSON'S discussion of "Ethics and Advocacy" in the December, 1944, issue of this Journal has made it clear that, among the competing moralities in the world, the one of which his writings form a part does not make any demand on people to participate in it, does not attempt to recommend itself; that those who do participate in it do so less by choice than as a result of habit and causality; and that consequently that element of advocacy which, in my discussion of "The Meaning of Good" in the same issue of the Journal, I said his position contains, is not really there, but was imported into it from the relativist ethical background from which I criticised it.

Anderson's claim, however, to demonstrate the impossibility of working out a "consistently relational view of goodness", seems to involve a similar importation of his own realism into the position of his opponents. It is true that a quality is introduced in my own phrase, "those things which are considered desirable within this movement" (p. 172; quoted by Anderson on p. 180), but this phrase was not intended as an expression of a "consistently relational view of goodness", but as a description of the relational element which I said was in uneasy and unknowledged alliance with realism in Anderson's own position—a charge which he has shown to be based on a misinterpretation on my part. Anderson's importation of his own realism into his formulation of relativism appears in his assumption that the latter is bound to define goodness as "the desired", either by the speaker, by the hearer, or by everybody. (So, for example, in his analysis on p. 186 of what would be involved in "recommending" inquiry.) A consistent relativism would not treat ethical sentences as

descriptions of demands rather than descriptions of anything else, but would treat them as *themselves* demands rather than descriptions of anything at all. A demand, indeed, is partly a description of a wish; but it is partly also a direct appeal to the hearer's emotions or (if the existence of such a thing be admitted—it need not be) his will. The description is needed to indicate the effect which the speaker desires to achieve by the second element; but it is this second element which, on this view, alone makes the sentence an ethical one. We may call it (borrowing a word which Anderson suggests in connection with theology) the element of "incantation". A science of ethics is impossible on this view, not because it is impossible to present the view consistently, but because a science cannot be constructed out of incantations.

The one-sidedness which Anderson has effectively shown to have coloured my previous formulation of the issues also led me to ignore the fact that the consistent relativist who continues to use such terms as "good" and "ethics" is just as liable to be misunderstood as Anderson is. His use of "good" as pure incantation is as far removed from common usage as Anderson's purely descriptive use of it; and an element of description will be imported into his usage from the presuppositions of his readers as readily as the element of demand will be imported into the usage of Anderson. Common usage, like that of Moore, and unlike that of Anderson on the one hand and such consistent relativists as Ayer or I. A. Richards on the other, is either frankly or covertly inconsistent; and the consistent relativist has in this respect no more right than Anderson has to consider his usage *the* continuation of the common one. He has, however, no less. Anderson rightly insists that the discovery of this inconsistency in common usage need not lead him to abandon the use of the word "good" altogether; he is perfectly entitled to go on using it consistently in one of its various inconsistent common uses. So, however, are Ayer and Richards. What is involved here beyond the selection of different conventions?—it being granted, as my previous criticism did not make it clear that it *was* granted,

that neither convention originated entirely with Anderson or with Ayer, both being partial continuations of the common usages.

It must be admitted that if the consistent relativist continues to use the word "good", and not just to describe its use by other people, he can only use it as an incantation; if there is no place for incantations in his morality, he cannot use it at all. To this extent, supposing both Anderson and his relativist opponent to be working within the same morality, there is more reason for Anderson to urge the relativist to drop the term altogether than for the relativist to urge Anderson to do so. Anderson's positive ethics does not involve a departure from the anti-incantative morality which both parties may very well share; it is certainly a discussion of a real subject. It does, however involve him in arbitrariness at another point.

In all moralities there are real things described as "goods", things which may be studied by positive science, even if in some moralities their description is also used as a directive pointer for incantations; but Anderson only retains the description "goods" for the things recognised as such in a particular morality. It may be argued, however, that this is the *only* morality in which the description of its activities as "goods" is not meant to convey a demand; and so this is the only use of the term that is consistently descriptive—the activities described as "goods" in other moralities would not be so described if they were not being "recommended". This belief is suggested by Anderson's contention that the science of ethics is concerned with "descriptions which the admixture of recommendation confuses" (p. 179). The appearance of arbitrariness would also be removed from Anderson's usage by the belief that all the activities which go on best *without* recommendation assist one another, and so have a "real solidarity" (p. 187). This is perhaps just another way of saying that they form one and only one morality or movement, and if the common name that has been given to the activities united in this movement is the name "goods", there is no reason why that name should not be retained for them, even if it is

used in other ways by other people. If I have not misunderstood Anderson again, these are in fact the grounds of his usage, and the reasons why he does not regard the usage as an arbitrary one.

This belief that disinterested activities cannot or do not conflict is what I described in my original criticism as "a restatement of Kant's criterion of non-contradictoriness". To this Anderson replies that he sees nothing "contradictory" in the fact that "evils are found opposing other evils" (p. 185). Neither, however, did Kant; but he did see something contradictory in the situation of goods opposing other goods, and saw in it a sign that something was being put forward as a good that was not really one at all; and so does Anderson. The instance I gave to refute the theory in Anderson's form of it was that of disinterested inquiry contributing to its own destruction, by which I certainly did not mean merely the destruction of a particular inquirer. The means of putting an end to disinterested activities are considerably more potent than they were at the time of Socrates, and they have become so partly as a result of disinterested inquiry. I shall attempt to restate this criticism in a less objectionable way.

There is such a thing as disinterested destruction as well as disinterested production and inquiry. There is even a *morality* of destructiveness which spreads by "going on" rather than by attempting to recommend itself—which, that is to say, is "confused by the admixture of recommendation". It is never found in a quite unmixed state, but as Anderson admits (p. 186), neither is any other morality. It is true also that protective measures, including preceptive morality, designed to curb and confine this destructiveness, often simply provoke it, and curb and confine production and inquiry. But the destructiveness is not simply brought into being by these protective measures; on the contrary, one reason why the protective measures provoke it is that they are themselves partly inspired by it. (So Freud, in "Civilisation and its Discontents.") Nor is there any reason to suppose that with the removal of such protective measures against it, the destructiveness would simply wither away. In fact, the morality of



destructiveness appears in its purest forms, the forms least "confused by the admixture of recommendation", when men are driven to despair and all moral precepts appear to them a mockery. Disinterested inquiry may assist this disinterested destruction, not only by furnishing it with new material weapons, but also by its criticism of the authority of moral precepts and removal of inhibitions, and in this it contributes to its own destruction. Certainly disinterested destructiveness destroys itself along with everything else within its reach, and only continues to exist through the intervention of alien "interested" forces; but my point is that this is also true of disinterested production and inquiry. At least two non-preceptive moralities are possible, and both of these may oppose, not only one another, but themselves.

I suggested that Kant's confidence at the corresponding point in his ethics might be accounted for by his theological "postulates"; and also that such theological doctrines as original sin might be the historical source of the view that "inescapable deficiencies of all human language" prevent the consistent formulation of the notion of "intrinsic mandatoriness". I described this historical connection in such a way as to suggest that theological doctrines may be *inferred* from the proposition that human language has such deficiencies. This inference certainly cannot be made. But that this proposition is at least a historical residuum from theological beliefs formerly held, seems particularly likely in the field of ethics, in which the thing which cannot be consistently expressed is an identity in meaning between "the objectively good" and "the demanded", a problem first discussed in connection with the demands of God. ("Does God's demanding a thing *make* it good, or does He demand it because it *is* good?") Anderson is, of course, quite correct in insisting that "if theology is any sort of doctrine and not just incantation, it, with all other doctrines, will be undermined by that recognition" (i.e., the recognition of inescapable deficiencies in human language)—"which, however, is itself a doctrine", and so, I take him to mean, is undermined by itself (p. 176). My concluding advice to the theologian was precisely that he should admit this, and that he should under-

line the fact that the recognition is "itself a doctrine" by stating it as a such, and as a specifically *theological* doctrine—with its own specifically theological qualification, and no other. He should say, in short, something like this: "*Of course* theology is 'undermined' along with all other doctrines; for, along with all other doctrines, it is a human work standing under God's judgment. But it is also *upheld*, along with other doctrines, by His mercy." If theology is capable of being "justified" in any way, it can only be justified theologically, not philosophically.

Anderson makes clear his attitude to this way of dealing with the inconsistencies of common ethical language, and can certainly not be accused of appealing to "inescapable deficiencies of human language". There are, however, echoes of theological doctrines in other parts of his discussion, particularly in his concluding reference to the "real solidarity in goodness" which may be caricatured by evil. This is reminiscent of the theological doctrine of a divinely given "order" in the world, of which men or devils may produce "spurious imitations". The Russian theological moralist Vladimir Soloviev, for example, contends in his "War and Christianity" that the whole notion of "Antichrist" may be summed up in the proverb, "All is not gold that glitters". The English Christian Socialist F. D. Maurice develops in a letter to J. M. Ludlow a contrast between "God's order" and "man's systems", and corresponding to it a distinction between "diggers" who seek to discover God's order and "builders" who attempt to set up ideal human systems. The resemblance with Anderson lies not only in the conception of the mimicry of good by evil, but also in the conception of the true good, "God's order", as a social reality which exists and maintains *itself* against both direct opposition and the patronage of "moralists", in the sense not of students of ethics but of upholders of a Pharisaical ethical viewpoint and of a solidarity based on censoriousness. The propositions in which such a theological ethics is formulated, i.e., in which "God's order" is described, are not of the form "X would be a good thing", but of the form "X is the case", e.g., "The Son of God has become man, and made men

the sons of God". But the "order" which these propositions describe, as the instance given indicates, is primarily made up of personal relationships, and it is considered not only to give rise to minor demands within itself in the same way as the activity of research gives rise to demands within itself for equipment, but the very fact of its existence is regarded as itself a demand upon all persons to take up, by acts of personal trust, their place in the order—a place which, however, they are to "trust" that they in some sense already occupy; "inescapable deficiencies of language" beset the theologian here.

Anderson would not, I take it, be concerned to deny that the elements of a genuine science of ethics are present in his theological as well as his other predecessors; but he would rightly insist that his use of such a starting-point does not imply that anything which can intelligibly be termed "theology" remains when he has "disentangled the real subject (and the recognised truths about it) from the false accretions" (p. 176). Although "disinterestedness" and "personal trust" are states of mind which have their similarities, he makes it clear that for him "personality", with all its paradoxes, is neither a value nor a fact. That he does not regard it as a fact is plain from his discussion on p. 183 of the question, "What are 'we'?"; and for him that already settles the question whether it is a "value"—there is no place visible in his morality for the kind of fiction by which E. M. Forster, in his essay "I Believe in People", "assumes", "for the purpose of living", "that the personality is solid, and the 'self' is an entity", and "ignores all contrary evidence", and says in the same essay that although the assumption that men are immortal and that society is eternal are both false, "both of them must be accepted as true if we are to go on eating and working and loving". Yet his confidence that the spurious goodness *cannot* destroy the genuine, and that the genuine cannot actually help it to do so, seems to be either just such a fiction, or a "postulate", an "act of trust", which arises more from the old theological than the new scientific conception of ethics; while at the same time it is on this very postulate of the solidarity of the good that his "science of ethics" is based.

## II.

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The One Good.

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By JOHN ANDERSON.

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WHILE Mr. A. N. Prior admits having misinterpreted my position on certain points, he seems to attach little weight to what I regard as the major points in my "Ethics and Advocacy" and so, in my judgment, he goes off into side-lines. Of course, if he sees no force in my distinction between the "real" and the "spurious" solidarity, he will not be inclined to pursue the hypothesis that in the conflict and confusion between these two lies the explanation of the confused state of ethical theorising. But at least he should give me credit for *thinking* that the distinction is sound, for thinking that, in however summary a manner and with whatever barriers to "getting it across", I am presenting *what is*.

In fact, in taking my position to rest here on a "postulate", Prior would seem to be resurrecting the charge of "recommendation" of which he had previously absolved me; if I am not now directly making demands of other persons, I am making demands of the facts—holding that they *have* to be of such and such kinds—and this would, incidentally, involve making demands of persons, viz., that they should see the facts in that way. But even at the beginning, in speaking of the "morality" of which my writings form a part, Prior shows that he has not really given up thinking of me as advocating something. He could do so, indeed, only if he admitted with me that ethics is just as positive a subject, just as definite a field of study, as physics; failing that, it can only be a variable matter, depending on people's choices, usages, postulates, and what I in particular write on what I call ethics will be merely a presentation of "my" ethics, of the usages, and eventually of the ways of behaving, which I prefer.

The main point of my fairly detailed remarks on inquiry was to show that consideration of "my morality" (or my



adherences) was irrelevant to consideration of the actual content of my argument, that inquiry is a subject, and that, even if a man in discussing that subject is himself inquiring, it is obstruction of discussion to turn attention to his activity and away from what he says. Even if he regards "devotion to truth" as having characters, and relations to other devotions, not recognised in common opinion, he is not to be understood as laying claim to a specially high degree of such devotion; and neither his devotion nor his backslidings *as his* have anything to do with the question. But, further, even if the subject under discussion were "adherences", and if a man in what he said about adherences showed in some measure his own adherences, it would still be a side-tracking of discussion to reply to what he said by commenting on what he revealed about himself.

What I have said, then, is that there is a subject *good*, and that what Prior takes as different ways of using a word are different misapprehensions of this subject. And this could be expressed by saying that there is only one thing that is "meant by" good, however confusedly some people may apprehend it. In the same way I should say that there is only one thing that is meant by *mind*, and that, even if some people define it relationally as "what knows", it is *it* that they are thus wrongly defining. Prior speaks as if it were in some quite accidental or arbitrary way that an "inconsistent usage", in which quality and relation were run together, had sprung up in common speech and thought, and as if some persons, seeing the inconsistency, then decided equally arbitrarily that in their "usage" it would be simply the quality or simply the relation that was referred to. But if it can be shown, as I think it can, that the common usage distorts a real subject and that the distortion arises quite naturally in the conditions under which the subject exists, the removal of the distortion will also not be a matter of simple "choice" but will follow a definite line—which will not be the relational line.

At any rate, I do not think Prior has had much success in showing what a "consistent relativist" could say. If an

"ethical sentence" is not merely to convey information as to the object desired by the speaker, but also to be an appeal to the hearer's emotions or will, it can only be because the element of "incantation" is also informative. Failing that, how could it possibly influence the hearer? No one, presumably, would say that "X is good" influences a person towards X more than is done by "X is bad", simply because he likes the *sound* "good" better than he likes the sound "bad". The influence would depend on his associating the former with certain activities or "ends" and the latter with certain others. Similarly, if a man were influenced in different ways (or influenced at all) by "Revolution! Rah!" and "Revolution! Bah!", it would be because the monosyllables conveyed to him something of the speakers' attitudes to revolution—this, of course, impinging on his own previous attitudes to revolution and to the speakers. In other words, the supposed incantation is charged with unstated "values"; they are its "meaning". I have acknowledged, of course, that there is a great deal of ambiguity and confusion in such appeals; but always there must be something positive, and always, I contend, when disentanglement has taken place, good will be found to be part of that positive content.

To show that there is one type of activity which has been the "real" subject of all moral theories whatever, would obviously be an immense undertaking. Nevertheless, that is what I believe to be the case; and I do not decide to call the special forms of that general type "good" because they proceed best without recommendation or because they all hang together or make up a single "morality"—or for any other reason than that I think they *are* good. At the same time, I think that Socrates and others, into whatever moralistic confusions they occasionally fall, have seen truly that goods do not conflict with one another. Prior speaks again, in this connection, of disinterested inquiry "contributing to its own destruction". The first question to be raised here is that of "contributing". If an X attacks and destroys a Y only because it *is* Y (so that, if it had not been Y, it would not have been destroyed), is

that any ground for saying that it contributed to its own destruction? To say that goods co-operate and propagate themselves in special ways is not to say that nothing is inimical to them and that they cannot be destroyed; but being destroyed by what is opposed to goodness is not being destroyed by goodness. However, Prior seems to be suggesting cases in which, but for Y, there would not have been an X to attack and destroy it—and that brings us to the second question, what is and what is not “disinterested” inquiry.

I take it that the “atom bomb” is an example of the weapons which are furnished by inquiry to destructiveness—weapons the use of which may destroy civilisation and thus inquiry itself. It seems to me quite possible that we have entered a period of cultural degeneration, that we are approaching one of Vico’s “new barbarisms”. But while that in itself does not mean the end of civilisation, I should maintain that far more potent forces than “scientific weapons” are at the back of the decline of culture. And what is particularly to the point is that “scientific advance” has been largely bound up with the decline of inquiry, that modern science does not exemplify disinterested inquiry. Its spirit has been “practical”, it has been concerned with “getting things done”, with facilitating transformations and translations, not just with finding out what is the case and with the “criticism of categories” that that involves. It has served “society”, i.e., that false solidarity of group interests of which I previously spoke; it has not been disinterested *or philosophical*. And while I should have expected Prior to take a less simple view of science, to see how far it merely imitates inquiry, I also find a certain simplicity in his theory of “disinterested destructiveness”. He gives his case away, I think, in his reference to despair; despair is not a disinterested but an egoistic attitude, an elevation of the particular above the general—which is the weakness of all the “spurious” creeds.

Now it cannot be said that theology escapes this charge; in so far as it is a doctrine of a universe or system of things, it is “solidarist”—it tries, like the egoist or the patriot, to set

up something whose value resides in its "unity" and not just in its character. But in so far as it criticises lesser unities, in so far as it opposes "the world" and "scientific" optimism, it is a closer imitation of the real thing than other views are. Taking "original sin" as signifying the worthlessness of the individual, we can regard it as making some approach to the recognition of those *causes* which, as I suggested following Croce, in "The Meaning of Good" (Journal, September, 1942), are the real subject of history—or of culture, or of ethics. And theology (or religion) may be closely connected with those "myths" which, on Vico's view, are the first approach to an understanding of culture.

But while a positive view of ethics may develop in this way and may continue to find more in common with theology than with melioristic science, it is not theological and it has not even that long-range optimism which goes with any belief in a "system" of things. When I argue that goodness cannot be eliminated by the "spurious solidarity of social unity", I take this to be a matter of fact; I take it that goodness (but likewise evil, and likewise pretence) is coeval with society. But I certainly do not take society to be eternal—though I see no possibility of fruitful inquiry into the conditions either of its ending or of its beginning. And I cannot see, in this connection, how Forster (who presumably has gone on eating, etc.) can accept as true what he has just said to be false.

I am not attempting in these discussions to give more than a sketch of a position. I cannot say just at what point, and with reference to what background of study, linkages would emerge and the position would appear other than arbitrary to hitherto dissenting readers. But I hope I have shown, with regard to Prior's discussion, that he has not established either that goods, as I have presented them, can conflict or that there is more than one meaning of "good". For the rest, the sort of "imitation" I have referred to, the ways in which "interests" masquerade as disinterested (and the consequent confusion affecting the study of disinterestedness), should be obvious to all those who admit that such a thing as disinterestedness exists.



## REVIEW ARTICLE.

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### THEORY AND PRACTICE IN THE SOCIAL SCIENCES.

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By P. H. PARTRIDGE.

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IN recent years, a considerable volume of work has appeared dealing with the methodology of the social sciences. Much of it has been exceedingly critical of some of the assumptions and methods of research that have established themselves in these fields. In part, this is a natural and useful reaction to the rapid but quite uncritical growth in the study and teaching of the social sciences which has been taking place for some time inside and outside the universities of most countries. But one development which has contributed a great deal to this interest in the character and methods of the social sciences is the appearance of a general sense of social crisis, and the spread of the ideology of social planning. Along with the growing popularity of the notion of a scientific control of social development, the belief has been gaining ground that the social sciences in their present condition cannot provide the guidance and control which social reformers and planners consider to be both possible and necessary.<sup>1</sup>

Professor Walker's recent book<sup>2</sup> is a manifestation of this particular movement in the social sciences. Though he is wholly concerned with the present position in economics, he employs many of the arguments which are being currently applied in the other social sciences, and raises many questions

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<sup>1</sup> For an important elaboration of this view, see R. S. Lynd: *Knowledge for What?* (Princeton, 1940). Also K. Mannheim: *Man and Society in an Age of Reconstruction* (London, 1940). A discussion of some of the main issues involved is to be found in K. Popper: *The Poverty of Historicism*, in *Economica*, Vol. XI, Nos. 43 and 44 (1944).

<sup>2</sup> E. R. Walker: *From Economic Theory to Policy* (University of Chicago Press, 1943).

of great importance for the social sciences generally. The book is an essay on the application of economic theory to the concrete problems which occupy the practical man and the statesman. The author's experiences as an "economic adviser" to governments have convinced him that much of the existing body of economic theory has little relevance to the social conditions and problems confronting the statesman. For this "gulf between theory and policy", Walker considers economists to be largely to blame. His chief purpose, therefore, is to consider what changes need to be made in accepted economic theory and in prevailing methods of inquiry in order that the conclusions of economic theory should be more useful to those charged with the determination and administration of policy.

Walker maintains that the chief defect to be found in the theories and procedures of economists is what he calls "theoretic blight" (chapter 4). Economists, either from a "love of pure theory for its own sake", or because a more "realistic" procedure would involve the scrapping of many assumptions and raise problems with which—given their existing resources—they are unable to deal, persist in working out the implications of assumptions or hypotheses, despite the fact that neither assumptions nor hypotheses are clearly applicable to the actual conditions of economic life. Theoretic blight is defined as "the development of economics under the impulse of theoretic construction for its own sake along paths which become ever more remote from the real world" (p. 57). A considerable part of the book is devoted to the criticism of what he considers to be important cases of theoretic blight in the work of his eminent predecessors and contemporaries.

In general terms, "theoretic blight" appears to mean, in part at least, the procedure which in philosophy has been called "saving hypotheses".<sup>3</sup> According to Walker, it manifests itself in the psychological theories underlying much accepted economic doctrine; he argues that economists have tended to ignore the operation of other motives apart from "the desire

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<sup>3</sup> The phrase is Professor John Anderson's.

for financial reward". Again (chapter 6) they have tended to concentrate upon the study of the market, ignoring the existence of "extra-market operations", even though it is frequently impossible to understand or to anticipate the behaviour of the market without taking these extra-market factors into account. In this chapter, which the author evidently considers to be the most important and original in the book, he tries to work out a coherent theory of "extra-market operations".

The other feature of the book of most general interest is that the author, in line with a number of recent writers on the social sciences, rejects the conception of the ethical neutrality (the *Wertfreiheit*) of economics and the other social sciences. He argues against the view, which had become a commonplace among social scientists, that it does not fall within the province of the economist (or social scientist) to pronounce upon question of "values" or to give a lead to policy.<sup>4</sup> He appears to hold that the striving of economists for moral and social neutrality is one of the main reasons for the inadequate contribution economic theory at present makes to the solution of issues of policy. Accordingly, in the last three chapters of the book, he sets out to criticise the notion of the "neutrality" of economics and to consider what light economists may throw on the outstanding practical problems of the day.

### Theory and Its Application.

In chapter 3 (*Towards Realistic Economic Theory*) Walker undertakes a general discussion of the nature of theory and of its applicability to concrete situations and problems. He attaches much importance to a distinction between "pure theory" and "the analysis of concrete situations". The pure theorist "may never consider concrete problems"; while "those

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<sup>4</sup> Another recent manifestation of the same current is to be found in G. Myrdal's great study of the negro problem in the U.S.A.: *An American Dilemma* (New York, 1944), Appendix 2: *Facts and Valuations in Social Science*.

who want to use theory for the discovery of concrete truth" may be distinguished from those dominated by "the impulse of theoretic construction for its own sake". In all this, it is not explained how, apart from the study of some "concrete situations", the pure theorist would ever be able to conceive any hypothesis at all. Walker relates this distinction to a more fundamental one between "empirical generalisations", which "affirm certain conditions as part of the real world", and "the analysis of the implications of a given set of conditions", which merely "affirm logical relations between certain conditions". We are given to understand that "logical relations" between different sets of conditions are distinct from relations "in the real world".

However, this distinction will not bear the weight that Walker seeks to put upon it. It amounts to no more than the distinction between the assertion of a *proposition* which the theorist believes to be true (whether or not he has personally observed it) on the one hand, and the assertion of an *implication*—of the *fact* that a certain conclusion follows from certain premises. It is difficult to believe that any serious economist has been "dominated by the impulse of theoretic construction for its own sake" in Walker's sense: that is, that any economist bothers to work out the implications of premises without his having any interest in the question whether the premises or the conclusions are *true*. If an economist considers it to be worth while to discover what conclusions may validly be inferred from given premises, either he believes that the premises are true and therefore the conclusions will be true also, or he wants to use the conclusions in order to test the truth of some of his premises. (We leave out of account for the time being the question to which Walker and other economists have given much attention, viz., whether anything is to be gained by persisting with assumptions which are known to be only very "approximately true".) In either case, the only question the theorist is interested in is the question, what is the truth (in Walker's language, what conditions "are part of the real world")?



"The test we must apply to economic theory, therefore, is not whether its results are contained in its premises but whether it is a serviceable instrument in the study of concrete problems" (p. 47). This is nothing but a clumsy way of asserting the logical commonplace that conclusions, though they follow validly from given premises, are not necessarily true—the premises (or one of them) might be false. Again, Walker is saying no more than that the test of a theory (hypothesis) is the truth or falsity of the conclusions inferred from it. Has anyone ever doubted it? An economic theory (a proposition or set of propositions) asserts that something is true, that something is a "concrete situation". And that is so, whether the theorist arrives at his assertion directly (e.g., from immediate observation) or mediately, by inferring it from premises.

Walker gives a number of examples from economic literature (pp. 36-7) to illustrate the distinction between "empirical generalisations" and statements which are "of a purely logical nature". This one is typical of all: the statement "when a trader or manufacturer buys anything to be used in production or to be sold again, his demand is based upon his anticipation of the profits he can derive from it" is of the first kind. It affirms "certain conditions as part of the world which the economist studies". In contrast, Taussig's assertion: "if a bank keeps just as much specie as it has notes outstanding, its note issue obviously can be no source of profit", is said to be of "a purely logical nature". What Walker is really trying to say is, that while the first passage is a *statement*, the second is an *argument*; that is, a conclusion ("its note issue is no source of profit") is inferred from premises not all of which, of course, are stated in the passage reproduced. But this is not sufficient to justify any distinction between "what is purely logical" and "what is of the real world". For Taussig is saying that a certain relation between the note issue, the specie and the profits of banks exists or occurs; at least, he is saying that when certain conditions are realised, certain other conditions follow. This whole discussion of what theory is, and what is meant by its application to "the real world" is a bad

muddle; it discloses at every point (and not only in the loose and fumbling language) an unfamiliarity with elementary logical theory.

An interesting question is: Why should Dr. Walker attach so much importance to what turns out to be the elementary distinction between statement and inference? Why should Robbins<sup>5</sup> think it necessary to assert such a truism as that "the changing facts which make prediction in any field possible must be discovered by observation or empirical investigation"? (This piece of profundity Walker matches with his "if theory is to be useful in the prediction and interpretation of events in the real world, it must be fed on facts".) One answer seems to be that economists writing on method are still haunted by the ghost of "rational", "a priori" science, "pure deductive systems."<sup>6</sup> There is the same suggestion of a distinction between "pure theory" and observed facts in the dictum of Marshall, of which Walker makes much use, to the effect that economics is "not a body of concrete truth, but an engine for the discovery of concrete truth". What Marshall appears to be referring to here is syllogism, and the distinction between

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<sup>5</sup> *The Nature and Significance of Economic Science*, quoted Walker, p. 40.

<sup>6</sup> Cf. Robbins, *op. cit.* (1st edition, pp. 106-108). "As we have already seen, the truth of a particular theory is a matter of its logical derivation from the general assumptions of the science. But its *applicability* to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation." "Here is a clear case where empirical studies bring us face to face with the insufficiencies of certain generalisations. And it is in the revelation of deficiencies of this kind that the main function of realistic studies in relation to theory consists. The theoretical economist who wishes to safeguard the implications of his theory must be continually 'trying out', in the explanation of particular situations, the generalisations he has already achieved." Note the wavering between two different views of the meaning of the "truth" of propositions; according to the second passage, observation may show a conclusion to be false, and its falsity consists in its not being in accord with what is observed, whether or not it has been "logically derived from the general assumptions of the science". I do not know whether anyone has described, in suitably plain terms, the logical looseness, the almost unmitigated ignorance of logical theory, disclosed in certain passages in this much discussed book. If not, I take this opportunity to fill a gap in the literature of economic methodology. Many economists, of whom Walker is one, discussing questions of methodology, do not go to reputable writers on scientific method but learn their logic from what other economists have written on the subject. So the blind lead the blind into the morass of logical confusion.

the universal and the particular proposition. "Theories" are universal propositions (i.e., assertions of a uniform connection between things of the sort A and things of the sort B)—these being as much statements of fact or concrete truth as any other propositions are, and equally capable of being discovered by observation. But if we mean by "concrete truth" propositions about some particular A, then we often arrive at such propositions by syllogistic argument in which our theories or universal propositions appear as premises. But, unless our theories (premises) were themselves statements concerning what is the case in the "real world", we could not infer from them true conclusions about particular concrete situations.

This distinction between economic theory and "concrete truth" (between "theoretical models" or "logical constructions" and "concrete situations" as Walker has it) serves no other purpose than to raise a dust about what is essentially a simple matter. If we hold as a theory or hypothesis that things of the sort A are (or are followed by) things of the sort B; if we then discover that a particular situation is an A, we conclude that it is a B. That the particular situation is an A is something we come to discover; and it may therefore be regarded as a part of our theory of such situations just as much as the proposition, all A are B. Thus, what is called the "application" of theory to concrete situations is not, so to speak, the superposition of theories upon "facts" or situations conceived as being of a logically different character from theories. "Application" is no more than the discovery of further premises which allow us to complete a syllogistic argument. In short, in "applying" theories, we are merely extending them further. The popular distinction between theories and the "facts" to which they are applied is logically indefensible.

### **Theoretic Blight.**

If these points be granted, it follows that there is much that calls for criticism in Walker's account of "theoretic blight" in economics. It arises, we are told, from "a love of theory for its own sake, and the desire to facilitate its growth in extent

and beauty" (p. 56). Now if (as we have argued) any theory is necessarily an assertion about certain facts, an account or explanation of some real situation, it is a curious charge that devotion to theory for its own sake (apart from any interest in its use or applicability) may divert the theorist from a study of the "real world". Of course, a theorist may be so devoted to an established body of theory that he will continue to ignore facts rather than sacrifice certain of his assumptions. Moreover, in most pieces of economic analysis or argument there are usually so many assumptions involved that even when a definite prediction is falsified, it may be impossible to show which of the assumptions were false. Thus, an economist might go on till the end of time "saving" certain of his hypotheses, if he wants to. However, in such a case the real point to be made is that the theorist is not sufficiently devoted to *theory for its own sake*, but allows considerations of convenience or professional pride (or some such non-theoretical motive) to impose a limit upon his criticism of received theory. Walker's account of "the causes of theoretic blight" (p. 57, ff) does succeed in showing that many economic doctrines have lingered on, despite repeated falsifications, because of the unwillingness of some economists to "face the wreckage of so much existing theory".

However, Walker includes within this conception of theoretic blight many different things, some of which are not open to objection at all. This indecisiveness is connected with a further point. The charge of being devoted to theory for its own sake is often intended to mean that a theorist allows his speculations to be guided entirely by his conception of what is purely of theoretical interest, while he neglects the problems that are of most practical importance. This, e.g., is one of the accusations R. S. Lynd makes against the social sciences, that they fight shy of the critical problems and tensions of contemporary culture and do not apply themselves to discovering how these problems can be overcome. Something of this sort is in Walker's mind (it is bound to be, since his chief object is "to bridge the gulf between theory and practice") : but throughout



the book he does not distinguish carefully enough between criticism of economic theory because it is false, and because it does not answer the sort of question the statesman or practical man wants to have answered. Frequently it is very difficult to discover from his text which of the two criticisms is being made.

This emerges, e.g., in such a passage as the following, where he is discussing the great variety of different market situations which exist, many of which economic theory ignores: "It is not suggested that all of these market situations should be made the subject of theories in the near future . . . the question [is], which should receive attention first. According to the views of Mrs. Robinson, the optimistic economist would tackle all those that responded to the existing technique and hope to devise new methods to deal with the others later on. The other method of attack, advocated here, is to decide which situations are most frequent in the real world and do one's best on them with existing technique. The next step is to build new techniques to handle the intractable cases. . . . Surely economic technique will develop more usefully if we do not confine it to problems in which existing methods work most readily. . . . *Perhaps it is necessary to add that frequency of occurrence in the real world is not necessarily the test of the practical urgency of studying a particular market situation. But it is a better guide than the tractability of the assumptions*" (p. 63, my italics).

But what precisely is the objection to the procedure of Joan Robinson's "optimistic economist"? If there *are* market situations which "respond to" existing "technique", this seems an obscure way of saying that the theories are confirmed by what happens in these situations. It is not a denial of the truth of the theories to say that there are types of market situations which are not accounted for by the theories; no error is involved here if the economist is aware—as Mrs. Robinson implies him to be—that there are other situations which are not covered by the theory. That there are other situations not accountable for by prevailing theory is all that Walker

claims in the passage quoted. But he appears to be confusing two different issues here; for, when he first referred to Mrs. Robinson's views (p. 58), his objection to the "optimistic economist" was that his assumptions were "unrealistic", "not applicable to the real world"—that is, false or meaningless, not a correct account of *any* real market situation. Dr. Walker, then, does not seem to be at all certain about what theoretic blight is. In general, he tends to confuse two distinct kinds of objection that may be brought against a body of theory, (a) that it is "unrealistic", meaning *false*; (b) that it does not include a theory of many situations which he considers to be important.

Moreover, as the passage shows, he makes no attempt to separate theoretical and practical importance. I do not think that anyone would question his view that economic theory cannot advance unless it wrestles with problems and difficulties. Taking his own example—market-analysis—if economists move on to consider types of market situation which are not included in existing market theory, they may be able to formulate laws of the market of greater generality and importance. What, then, is the issue between him and the economists he criticises? The answer is that in this attack on theoretic blight there are a number of separate issues which he mixes together. There is the question whether existing theory (that is, some parts of it) is an oversimplification or distortion of the facts it professes to describe or explain. It may be, e.g., that economists have stated quite generally laws or connections which hold only under very limited conditions: some of Walker's discussion suggests that this is what is in question. Again, there is the important question of the existence of economics as a *distinct* and *independent* science, and of its scope; the question whether economists, in order to answer the questions they have traditionally asked, need to attend to that wider field of reference which Walker considers that they should cultivate. This is an issue which crops up particularly in the chapters dealing with the psychological theories presupposed in economics and with extra-market operations—

but it is not separated out as sharply as it should be. A third question involved is that of the connection between theoretical and practical interests and the conditions of theoretical progress. In particular, whether his policy of "useful theories first" (p. 59) would in fact lead to the greatest advance in economic science. Walker does not often try to show that the problems which he brings to the attention of economists are of great theoretical importance from their point of view: it is sufficient for him if they are problems upon the solution of which large decisions of policy depend. It could be argued that, at least in many cases, if economists devoted their attention to the problems which Walker regards as being of "practical urgency", they would be turning aside from the direction in which greatest theoretical progress is to be made.

Before leaving this point, and as a preparation for what is to be said later, it is as well to say a word about the conception of theoretical importance. No doubt, the distinction between discoveries that are important for theory and those of mainly practical importance cannot always be sharply drawn. However, there is one obvious sense in which we speak of the theoretical importance of an hypothesis: namely, when we refer to the great range of distinct phenomena into the explanation of which the hypothesis enters. Thus, Marx's class theory is important, because of the way in which the class struggle (if we accept the view) enters into and affects most other social situations. In the same sense, we speak of the importance of Freud's theory of repression or of the theory of natural selection. But discoveries of the greatest theoretical importance *may* be of little practical importance; they may neither increase our ability to control situations, nor even our ability to predict the occurrence of events which we particularly want to be able to predict.

### **Prediction in the Social Sciences.**

In considering the shortcomings of existing theory and in making proposals for their correction, the criterion to which Walker seems to appeal most frequently is ability or inability

to predict. Thus, in suggesting to economists that they go out and establish their own psychological foundations (if established psychological theory does not contain what they want), he uses the argument that "any advance in the direction of realistic analysis will increase the predictive value of economic science and diminish, even if only slightly, the scope of personal opinion" (p. 93). Indeed, one of the main reasons for the present "gulf between theory and policy" is the low predictive value of much existing theory; and it is chiefly in order to increase the predictive value of economics that Walker wants economists to include the study of many aspects of social behaviour that have traditionally been excluded from the scope of economic science. It is suggested that the inability of economists to predict better is largely due to their abstracting from certain social factors (psychological, sociological, political, etc.) which enter into "economic" situations (as these have been traditionally defined) and condition "economic" factors. This argument also raises methodological issues of general importance for the social sciences.

It is not strange that Walker should put such emphasis upon ability to predict. As we noted, his tendency is to identify the distinct conceptions of theoretical and practical importance; prediction, possessing both theoretical and practical significance, allows the identification to be easily made. It is not necessary to remark upon the practical significance of ability to predict. On the purely theoretical side, power to predict is usually accepted as a mark of theoretical perfection (as, e.g., in astronomy or physics, as contrasted with any of the social sciences). It is true that when a scientist can predict with the certainty and precision of the astronomer, this means that his theory is both precise and relatively "complete"—that is to say, it does not leave out of account any of the factors which enter into the determination of the phenomenon we are interested in anticipating.

However, from the point of view of the growth of theory, it is usual to attach more importance to ability to predict than it really possesses. To begin with, scientific prediction



is merely a case of inference; the reference to the future involved in prediction is of no theoretical significance. Further, any true theory (proposition or set of propositions) taken in conjunction with appropriate observations enables us to predict something with certainty, though what we can predict may be of small or no practical interest to us. On the other hand, however exact and carefully worked out a theory may be, there are always characters of the phenomena whose occurrence we want to be able to anticipate which we cannot predict. Commonly, we tend to overlook how limited the power of the physical sciences to predict really is, because, in general, we are more interested in the characters of an event which they can predict than in those which they cannot. Whether we regard it as detracting from the importance of a theory that it does not enable us to predict will always depend upon our practical interests; i.e., on what, for one reason or another, we should like to be able to predict. Of course, it is quite another matter when a theory leads us to false predictions.

This much being prefaced, it can be argued that the policy of "useful theories first"—if this means the subordination of theoretical interests to the desire to be able to predict—will not necessarily conduce to the greatest theoretical progress. Sometimes, inability to predict may be indicative of a serious gap in the working out of a theory; thus, *ceteris paribus* qualifications (where other things are never or seldom equal) provide a problem and stimulus for further investigation. Here it is a question of discovering how given factors behave under a variety of different conditions; in other words, of discovering new laws or regularities. On the other hand, it is frequently the case (especially in the study of a social situation) that inability to predict is due not so much to ignorance of laws but to the impossibility or difficulty of discovering what are the various factors at work within the particular situation. In such a case, to concentrate upon the study of such situations merely because it is practically important to be able to predict their outcome would not lead to theoretical discoveries of general application and importance. But the

problems of prediction which confront statesmen and the expert advisers of statesmen are, to a certain extent, of that sort. Hence, it cannot be said that it would necessarily be to the advantage of economics or any other social theory for its development to be determined by the needs of practice.

Thus, there is always a tendency for the requirements of theory and those of policy to conflict, since (as we have said) the theoretical importance of an hypothesis is connected with its generality, while policy is bound to be deeply preoccupied with the specific and the local. Indeed, it might be said that an outstanding trait of most great theorists is their *detachment*, their ability to ignore the detail they would have to master if they were to be in a position to offer politically useful predictions and advice, their power of cutting through to the general and pervasive principle. Equally, it might be contended that one of the chief reasons for the slow growth of the social sciences as compared with the physical sciences is the inescapable entanglement of social scientists with practical interests, prevailing social values, local issues and conditions.

It will very often be the case, therefore, that where it is a matter of predicting what will eventuate in some particular social situation, a man with no theoretical qualifications will prove more expert than any theorist: he knows the intricacies and peculiarities of that particular situation as it would be of no special interest to the theorist to know them. It is worth noting too that the "gulf between theory and policy" is, in this sense, bound to remain great; there are special reasons why the power of the social sciences to make exact predictions must always be very circumscribed. This is a point which has been discussed by Hayek.<sup>7</sup> "The number of separate variables which, in any social phenomenon, will determine the result of a given change will, as a rule, be far too large for any human mind to master and manipulate them effectively. In consequence, our knowledge of the principles by which these phenomena are

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<sup>7</sup> F. Hayek: *Scientism and the Study of Society*, in *Economica*, Vol. XI, No. 41 (1944).

produced will rarely, if ever, enable us to predict the precise result of any concrete situation. While we can explain the principle on which certain phenomena are produced, and can from this knowledge exclude the possibility of certain results, e.g., of certain events occurring together, our knowledge will in a sense be only negative; that is, it will merely enable us to preclude certain results but not enable us to narrow the range of possibilities so that only one remains."

Now, the "number of separate variables" does seem to constitute a special difficulty for the social sciences. There is possibly some truth in a view that is widely held, viz., that the problems with which the physical sciences have been concerned have generally been simpler than most of the problems of the social sciences; the events they have been concerned to explain are, as a rule, functions of fewer variables. Because of this, and also because of the much greater possibility of controlled experiment in the physical sciences, they have been much more successful in isolating the conditions which are alone relevant to the physical changes or events which they want to explain. In the passage quoted, Hayek appears to exaggerate even the degree to which it is possible to discover "the principles on which certain phenomena are produced." Since, in most of the social situations we have to investigate, there are so many factors varying simultaneously, we are very rarely in a position to say with any approach to precision what are the factors necessary and together sufficient for the occurrence of a particular result.

Hence it is that social scientists have frequently to confine themselves to the assertion of "tendencies". One important example (because of its frequency) is that in which we know that usually factors X and Y are accompanied or followed by Z, but only under conditions which it is beyond our power definitely to specify.<sup>8</sup> Another form in which the difficulty

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<sup>8</sup> For a similar point, see C. G. Hempel: *The Function of General Laws in History* (The Journal of Philosophy, Vol. 39, No. 2, 1942). Hempel points out that when historians make an assertion concerning the cause of a particular event, a general causal law is assumed. But "it would often be very difficult to formulate the underlying assumptions explicitly with

appears is this: we constantly recognise the existence within a situation of opposing factors or forces; we speak of the "strength" of different factors and of the result as being determined by their relative "strength". This is what we often mean when we speak about a factor as *tending* to bring about a given effect, implying by the use of the word that we must not ignore the possibility of counteracting factors. No doubt, such a manner of speaking is not confined to the social sciences. But, for the most part, when social scientists make assertions of the kind, they have no way of measuring the "strength" of different factors, or even of saying what is meant by the "strength" of a social factor. What this means, in more general logical terms, is that it is rare for a social scientist to be in a position to specify accurately the conditions under which a given factor will, and those under which it will not, be followed by a certain result.

In criticism of Hayek's statement of the position, therefore, I should want to put less emphasis on the distinction he seems to make between our knowing the laws according to which various kinds of social events occur, and our being able to predict the outcome of a particular social situation—though that distinction is real and important too. The considerations I have advanced seem to me to show that the social sciences cannot hope to avoid the oversimplification, artificiality and tentativeness for which Walker castigates the economists; though, of course, this is no reason why social scientists should not constantly seek a greater precision in the statement of uniformities. Probably, too, there is truth in the point suggested in the last few lines of the quotation from Hayek. It is that social scientists find it much easier to determine some

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sufficient precision and at the same time in such a way that they are in agreement with all the relevant empirical evidence available. . . . Consider, e.g., the statement that the Dust Bowl farmers migrate to California 'because' continual drought and sandstorms render their existence increasingly precarious and because California seems to them to offer better living conditions. This explanation rests on some such universal hypothesis as that populations will tend to migrate to regions which offer better living conditions. But it would obviously be difficult accurately to state this hypothesis in the form of a general law which is reasonably well confirmed by all the relevant evidence available."



of the *necessary* conditions of the events they are trying to explain than to determine the conditions which are *sufficient* for them. Hence, they are able far more often to say that a given result cannot follow from known antecedents than to say that it will follow. Political science offers innumerable instances of the point; e.g., it is easy to point to necessary conditions for the existence of political freedom in a community, or for the successful working of a two-party system, or system of parliamentary government; but another matter entirely to say what would be sufficient. If we are considering the sort of service the social scientist can render to governments (which is Walker's theme), it is useful to bear in mind that, as Hayek points out, he will be able to say that something cannot be done more often and, as a rule, with greater certainty than to say that something can be done.

### **Abstraction and Particularisation in the Social Sciences.**

We have noted that, according to Walker, the "uselessness" of much economic theory, its inability to predict how a particular economic situation (e.g., a market) will behave or develop, arises in part from the fact that economists ignore, abstract from, certain conditions which are present or may be present in such situations. Thus, he suggests that one step necessary for "bridging the gulf between theory and policy" is that economists should analyse economic situations in their full complexity. In his view, one important instance of this failing is the way in which economists have commonly ignored the great variety of different psychological motives which may influence men in their market behaviour. It is not so much, he thinks, that economists have ignored the importance of psychological factors altogether as that they have worked with a false, a much too simple, theory of motives.

Here, of course, there is the question whether economic theory *needs* to make any psychological assumptions at all; whether it is not the case (as Robbins and others have argued) that there are general laws of the market which are true

irrespective of the psychological motives of those participating in market operations. In spite of what Walker says about theoretical blight, it is difficult to believe that the orthodox market theory which he denounces would have maintained itself unless it embodied generalisations which, whatever the particular psychological conditions, were at least approximately true. In any case, it is of the utmost importance, in the interests of theoretical clarity and progress, that economists should set themselves to discover what are the uniformities which continue to hold good under a great variety of different conditions, socio-psychological and other; even though this would no doubt divert them from the study of problems of more immediate practical urgency.<sup>9</sup> And, while it is important, both theoretically and practically, to examine what follows from certain market factors or variations under a variety of different social and socio-psychological conditions, the economist would never be able, within a single market theory, to comprehend or provide for all the different possibilities, all the different factors, which affect the results which follow from given changes in the market.

For this reason we could not attach much significance to the views of Mrs. Robinson which Walker quotes: "the funda-

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<sup>9</sup> Walker refers several times to his debt to the work of Talcott Parsons. He does not see, apparently, that Parsons attacks just the conception of the scope of economic theory which he is advocating. Cf. Parsons' essay on *Sociological Elements in Economics* in Becker and Barnes (eds.), *Contemporary Social Theory* (New York, 1940). Parsons takes the view that, although any social situation is a unity, "it can, like all other complex phenomena, be broken down for purposes of analysis into different factors. . . . The only way of maintaining a positive rôle for economic theory as a systematic *generalising* science is to make it the science of *one* of these factors in concrete human action, to be sure more conspicuous in those concrete activities we call 'business' than elsewhere, but neither confined to them nor excluding others there" (p. 638). Parsons adds that the economist will often venture beyond the borderlines of the subject of economics; "the important thing is not that he should stick to his own theoretical field, but that he should know *what he is doing when he goes outside it*". This last remark expresses my own objection to the procedure which Walker urges upon the economist. If economists take up questions at random, merely because they are suggested by practical requirements or because their solution facilitates prediction in a particular situation, it will not assist the establishment of fundamental generalisations in any one field of social inquiry. It is most likely to confuse the vital theoretical issues.

mental assumption upon which the present simple technique of analysis is based is that each individual person acts in a sensible manner from the point of view of his own economic interests. . . . [Now] if individuals act in a predictable manner, but from a wide range of complicated motives, the economist must await the verdict of the psychologist on what these motives are. Meanwhile, the optimistic economists are working out their analyses on the simple assumption, and resolutely refusing to despair of evolving a technique in the future which will allow them to assume the existence of whatever other human motives have an influence in the economic sphere".<sup>10</sup>

Clearly, any human motive may have an influence in the economic sphere (may affect the exchanges which men make of goods and services). But it is not clear why the economist should, in general, have to wait upon the psychologist to tell him what these motives are; many of them are evident to any observer of social behaviour. The reference to psychology only confuses the issue; it is not the absence of a mature psychology which retards the progress of economics, for, no matter how advanced psychology might be, the economist would still have on his hands the problem of showing how exactly the presence of a "large number of separate variables" would influence the result of any change occurring in the market. Moreover, it is oversanguine to assume that the psychologist will ever be able to predict the occurrence, and the relative strength, of a variety of motives in a complicated social situation; just as it is meaningless to suppose that there could ever be a single theory of the market (or a single "technique of analysis") providing for all the "human motives which have an influence in the economic sphere".

The economist ought not to be very worried by the fact that he cannot provide for all the factors that may possibly affect the outcome of a change in the market. What he might hope to do first of all is to discover certain general factors or tendencies manifest in any market (or in an important class of

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<sup>10</sup> Robinson: *Economics is a Serious Subject*, p. 10; quoted by Walker, p. 80.

markets). Obviously, such pervasive or general factors would be modified in their way of working by the special factors present in some markets but not in others. The features peculiar to a particular market—or special class of markets—would have to be dealt with separately (if, in any sense of the word, it seemed sufficiently important to deal with them): in any case, there could be no single, general, all-embracing theory of something called *the* market. Nevertheless, it will still be true that there are some common factors or uniformities exemplified by any market, and that these would need to be understood in order to explain the working of any particular market. And it may be argued that progress in the understanding of different types of market behaviour would be made only if economists concentrated to begin with on the discovery of common factors and types of connection between factors. That is, on establishing the essential or necessary conditions of a market, within which more special factors work.<sup>11</sup>

Now, as against this conception, we have the method which Walker recommends to the economists. His recommendation is that they should concentrate upon the analysis of “concrete situations”, important because of the frequency of their occurrence or for some such practical reason, taking account as well as they can of all the operative factors present in the situation. This he considers to be the only way in which the distortions and practical inutility characteristic of

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<sup>11</sup> This emphasis on the discovery of necessary conditions might be connected with Hayek's view (*supra*, p. 103f). Cf. Hayek, quoted by Popper, *Economica*, Vol. XI, No. 43: “economics developed mainly as the outcome of the investigation and refutation of successive utopian proposals”. Again, it is a matter of showing what can't be done; of demonstrating that conditions (which “Utopians” ignore or want to eliminate) are necessary for the situation they hope to establish. Thus Hayek has himself been trying to show in *The Road to Serfdom* that political repression is a necessary condition of social planning. Of course, the statement “repression is necessary for planning” is equivalent to “planning is *sufficient* for repression”. But this does not, I think, affect my earlier point that, as regards most of the events which social scientists are concerned to explain, it is easier to find some necessary conditions than to find sufficient conditions. Putting Hayek's point more generally, we might say that one important connection between practical social interests and the progress of social theory is that social hypotheses are often developed in order to debunk prevailing ideologies. The work of Hobbes, Savigny or Marx could be cited in illustration.



economic theory are to be avoided. And he is uncompromising in his opposition to the plea that the economist is bound to begin with oversimplification or approximation, hoping to correct these faults as theory becomes more refined. He takes this method to be one of the chief begetters of theoretic blight.

Would this concentration upon the "concrete" or total social or economic situation lead in fact to the more rapid advance of economic theory? And would it be to the advantage of the economist who is eager to give a lead in practical affairs? I believe that the economists whom Walker criticises are, in general, correct in contending that such concrete situations are "unmanageable" or "intractable". This does not necessarily mean what he takes it to mean, that "realistic assumptions" are intractable because they "conflict with the assumptions used in some of the established theories" (p. 58). There is also the point that, if we try to take account simultaneously of a large number of varying factors, we are not in a position to say what each contributes to the outcome of the situation. In dealing with a complex situation, such as the market, the economist's method—like that of any social or physical scientist—is bound to be selective and piecemeal. He must, that is, concentrate attention upon some factor or set of related factors common to situations of the type he is studying; he must begin by formulating a preliminary hypothesis concerning the connection between variations in factors he has abstracted from the total situation. It is only subsequently that he can go on to consider how the hypothesis must be modified when variations in other factors, so far ignored, are taken into account. Until we settle upon some hypothesis, we do not know what precise questions to ask in our examination of new situations: it is only when we have something tentatively fixed that we can consider the precise effects of one after another of a number of factors. It is impossible to see, then, how the theory of the market or of any other complex situation can advance except by successive refinements or approximations. We advance by making mistakes; one of the most valuable kinds of mistake is the

asserting of generalisations which we are later compelled to qualify.

Furthermore, it seems plausible to suppose that progress will most easily take place if the theorist moves from relatively simple to relatively complex situations, instead of plunging directly into the study of situations which occur "most frequently in the real world" irrespective of the difficulties they may present.<sup>12</sup>

This objection to the "abstractness" of the social sciences, their inability separately or together to do justice to the full complexity of concrete situations, is taken much further in Lynd's *Knowledge for What?* Lynd suggests that we should think not so much of separate social sciences, each possessing its own subject matter, but that all the sciences should take "the culture-continuum" itself as "the common subject of study". In particular, "the several disciplines as we now know them would be supplemented, and in part replaced, by [the study of] a series of specific problem-areas on which workers with all types of relevant specialised training would be co-operatively engaged. . . . Present departmental lines in universities would blur as training was reoriented around the full dimensions of problems rather than the traditions of disciplines and as research personnel of a variety that rarely at present join forces on any problem would build new patterns of research around these problem foci. The new field of child development, drawing together scientists from the biological and social sciences, suggests this new reorientation. Only by making use around each problem of a varied and co-ordinated group of specialists, trained to use their specialised knowledge *on that problem* and jointly to present that problem in its total setting, can science hope to fulfil its necessary task of

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<sup>12</sup> The essential points here seem to me to be well put by Wicksell in the passage quoted by Walker on p. 57. Walker's comment rides roughshod over the logical difficulties: "Economists who are not concerned with practical affairs may take a more complacent view [than he does] of first approximations and may consider time well spent in the elaboration of theories that are remote from reality, patiently waiting for the day when the approach to the world of concrete facts will be less difficult."

prescribing thoroughgoing analysis of *all* aspects of the phenomena it purports to study.”<sup>13</sup>

Lynd, like Walker, desires that in the teaching and study of the social sciences the emphasis should be put upon applied at the expense of pure social science. Of course, no objection is to be made if scientists choose to bring the resources of several sciences to the elucidation of situations which are considered to constitute or involve urgent social “problems”. Provided that we recognise always that this is necessarily a secondary activity; that it is not by concentrating on such “problem situations” that the sciences have made, or could make, great progress. Lynd is aware that each specialist brings to the study of a concrete situation a body of principles discovered independently of a detailed investigation of such total situations. It is obvious also that progress in the understanding of such situations would have to wait upon progress in the general field of each of the distinct sciences. For example, progress in the study of child development depends on discoveries that have been made and will be made in the field of general psychology; that is, in the science which concerns itself with the study of mental processes generally, whatever the special situations in which they are taking place and with which they are interacting.

It is not necessary to defend the boundaries that have traditionally become established between the sciences or to deny that there are important “borderline” questions ignored by all of them. Nevertheless there is an obvious logical reason why such subjects as “labour relations” or “child delinquency” are not the subjects of sciences (though, in many universities, the tendency is for teaching to centre around topics of that sort, along the lines Lynd suggests) but merely meeting-places for a number of sciences. Clearly, there could be no single theory of all the factors—psychological, biological, sociological, economic and so on—which enter into the kind of situation Lynd has in mind. Each type of factor has its own character, conditions and mode of operation; many of them

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<sup>13</sup> *Op. cit.*, pp. 166-7.

are simply indifferent to one another. The study of such situations involves the application to a particular case of general principles; principles which could only be discovered by abstracting the different sorts of factors and by studying each of them as it appears in a variety of different contexts. Before such a concrete "problem situation" could be shown to constitute a distinct theoretical field, two main questions would have to be considered. Do any of its characteristics that we want to account for involve any new question of principle or can they be explained by the application of principles already known? If they do, can those questions be settled by study of such situations themselves or would it be necessary to go beyond them and take in a wider field? In this way, we might determine the *theoretical* importance of the problems "practicalists" like Lynd and Walker worry about.

### Theory and Policy.

However, it is more with practical progress than with theoretical progress that Lynd (and also Walker) is concerned. We are to understand that certain social situations are "problem areas," not because they necessarily present acute theoretical problems to any of the social sciences and hold out the promise of fundamental discoveries of principle but because they involve social conflict and frustration.<sup>14</sup> Lynd would not distinguish between a theoretical and a practical motive for investigation; and he would hold that it is only when theorists are devoted to important practical ends that they will make contributions to theory.

He is not alone in this. According to Hayek "economic analysis has never been the result of detached intellectual curiosity about the *why* of social phenomena, but of an intense urge to reconstruct a world which gives rise to profound dissatisfaction".<sup>15</sup> Popper comments on this: "Some of the

<sup>14</sup> Cf. p. 169: "It would be salutary for us social scientists to ask ourselves, why are we caught at the present time with no *social science* professionals equipped to handle the acute and complicated problem of housing? And what similar problems confronting the culture likewise fail to fall within the boundaries of any of the present disciplines?"

<sup>15</sup> Quoted by Popper, op. cit.



social sciences, other than economics, that have not yet adopted such an outlook show by the barrenness of their results how urgently their speculations need practical checks. . . . Practice is invaluable, both as a stimulus and as a check to theory." Without denying the profound influence of practical demands upon the development of the sciences, especially the social sciences, we may still ask whether purely theoretical interests may not play a greater part than it is usual nowadays to admit, and whether the practical stimulus is usually as beneficial as Popper suggests.

It is certainly the case that once a body of theory has developed, it tends to generate its own problems. One case, e.g., is the sort of problem that arises when events are encountered which appear to conflict with accepted general laws; again, there is the sort of problem which consists in trying to demonstrate in detail the existence of a connection which is vaguely seen or guessed to exist. An example is Freud's attempts to establish connections between psychological mechanisms and social institutions. No doubt, as Hayek says, scientific curiosity is not *random* curiosity (as that of the young child appears to be); neither is it necessarily practical interests which give it its direction. The thinker who is immersed in a particular body of theory will frequently be able to distinguish the points at which great advances can be made; he can recognise in advance the problems whose solution will have important ramifications throughout the whole field of his science. It seems clear, for instance, that the interest of many physicists during the present century in the theory of atomic structure was dictated primarily by a perception of the theoretical importance and promise of the problems involved.<sup>16</sup>

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<sup>16</sup> These points, however obvious and commonplace, have to be made since many social scientists dealing with this topic have no inkling of any distinction between theoretical importance and the practical and moral senses of the word. Note how Lynd misstates the anti-practicalist position: "Values, they say, may not be derived by science, and therefore science should have nothing to do with them. Social science prefers to urge that all the fruits of scholarly curiosity are important, that there is more than enough work to do in filling in the infinite odd bits of the jigsaw puzzle of the unknown, and that science has no criteria by which to allot priorities in importance" (p. 181, my italics).

Admittedly it would be much more difficult, though it is not impossible, to illustrate from the history of the social sciences the way in which questions come to the fore because of a recognition of their theoretical importance. One reason for this is that the social sciences are as yet so undeveloped that even their fundamental categories or principles of explanation have not been established. Thus, in psychology and the other social sciences there are different "schools", taking their departure from different categories (e.g., "instinct", "behaviour") and explanatory principles. Owing partly to this lack of any secure foundation, it is inevitable that research in these sciences should be more or less random in character, consisting in the unsystematic investigation of whatever problems happen to arise. Thus, also, practical demands are able to play a particularly influential part in deciding what problems will occupy theorists at a particular time and even at a particular place. This I consider to be a constant source of weakness in the social sciences.

For one thing, a predominant concern with the discovery of solutions to practical problems tends to discourage far-reaching inquiry; there is always the inclination to put aside as too "academic" or abstract problems and hypotheses which do not appear to have a close bearing upon the problem in hand. A more important point is that the definition of a social problem which becomes accepted within the community usually involves many assumptions; e.g., assumptions are normally made about the interests or institutions which are to be preserved in the solution; about the factors that are to remain constant; and consequently about the general nature of the solution or the direction in which it is to be looked for.<sup>17</sup> But a developed theory would often show that many of these problems, in the form in which they are understood, are simply

<sup>17</sup> This incidentally is a point well made by Lynd: "It [contemporary social science] allows the powerful biases of the culture to set for it the statement of many of the problems on which it works. . . . On the positive side, it works in a general spirit of modest meliorism, seeking to make small changes for the better in the various institutions to which it applies itself" (p. 182).

insoluble. At the same time, the social scientist who accepts these problems as determining the scope and purpose of his inquiries will, as a rule, accept the assumptions implicit in them. It would not be difficult to illustrate from the history of the social sciences—especially from the history of political science—how constantly the influence of such problems upon theory has acted as a hindrance to free investigation. Marxism as a political theory has been generally ignored, including such an hypothesis as the “imperialist” theory of war, because if it were seriously entertained theorists would be compelled to reject as unreal or insoluble problems generally accepted as urgently requiring solution. Theorists who are devoted to an ideology, a movement, or to definite social objectives are not able to exhibit great freedom of mind in scrutinising the assumptions of their own social milieu nor great detachment in considering the assumptions of an opposing movement.

Popper’s point, in advocating the “technological” or practical attitude in the social sciences, is that the formulation of policies and the putting of them into effect fulfils the same function as experiment in the physical sciences. It would be foolish to decry the importance of trying to state hypotheses in what he calls “technological” form; of reducing social theories to definite assertions of what will, or what will not, follow from specified conditions. Nevertheless, the social scientist is not often in a position to carry out such experiments; he may make “practical” predictions, but even when he has the ear of governments, it is rarely that his proposals are put into effect in the form that he has prescribed. The social scientist is, for the most part, restricted to the observation of the policies of others, their conditions and consequences. Again, it is difficult to see how experiments which would test some of the most important social hypotheses could be contrived; e.g., it is hard to see how Laski’s well known view that “the working of British parliamentary government depends upon an agreement about fundamentals between the major parties” could be tested by any experiment which the theorist could deliberately devise; or again (to take one of Popper’s own examples) the “techno-

logical" law that "you cannot make a successful revolution if the ruling class is not weakened by internal dissension or defeat in war". With a great number of the quite fundamental hypotheses or problems of the social sciences, the social theorist is bound to rely on the observation, the theoretical study, of the working of policies, as distinguished from the theorist's own participation in the making and execution of policies. I consider, therefore, that Popper exaggerates the importance, for the growth of the social sciences, of the connection of theory with practice in this sense.

However, the main point is that, even granting the value of the experimental, technological approach to the social sciences, this would not show that it would necessarily be to their advantage to take their departure normally from questions of how (or whether) prevailing social objectives could be achieved. Often, interest in such a problem may be a stimulus to theory. But even where a theoretical problem is first approached in that way, theoretical progress usually requires that the practical interest fall into the background; the effort to formulate an important hypothesis precisely and to verify it will take the investigator far beyond the "problem-situation" from which he started. Marx's class theory is sometimes cited as an instance of an hypothesis arising out of a strong practical interest. There seems to me to be little doubt that it was a belief in the theoretical importance of the conception that led to the working out and to the variety of applications which it received in the hands of Marx and Engels. And in this connection it is particularly important to point out that Marx's attempts to elaborate and apply the theory would probably have led to far better results if he had had less of a practical interest in it; in particular, if he had not been so eager to *make it come true, that is, to bring about the events which would verify it*. That brings out one of the main sources of theoretical confusion in the mingling of theoretical and practical interests: the theorist-practitioner in social life is not merely content to say: "If we do X we shall secure Y" and then wait to see if the expectation is realised. He usually can-



not refrain from meddling with other conditions as well in an attempt to get the right result. Theory requires a personal indifference to the results of experiments which is often incompatible with strong practical or social attachments.

Many contemporary social theorists not only believe that the main business of the sciences is to consider how current "problems" are to be solved or prevailing objectives realised; they hold that it is within the province of the sciences to say what objectives should be sought. This is Lynd's view; it is also Myrdal's.<sup>18</sup> The latter argues at some length that the work of the social scientist cannot escape being affected by his valuations; what is necessary, therefore, is not that he should try to exclude valuations, but that he should seek always to make them quite explicit. What the point of this would be, it is impossible to see. By his making valuations explicit is presumably meant disentangling his moral beliefs from his economic, psychological or whatever it might be. If this disentanglement is successfully done, the valuations are of no importance and need not be stated at all; it would be possible for a critic to consider the truth of the economic or psychological statements without worrying about the valuations. On the other hand, if the moral assumptions are explicitly stated for the purpose of using them to draw practical (moral) conclusions—this seems to me to be clearly what Myrdal has in mind—then the economist or psychologist has simply entered the field of moral theory. In that case, a purely arbitrary or dogmatic statement of his valuations (moral beliefs) is no contribution to ethics or to any other social science.

Walker, although he believes that economic theory in the past has been strongly influenced by economists' interests in practical social objectives, that this connection is necessary and desirable, tries to avoid the conclusion that economists as such can advocate "values" or ends. "The student who approaches economics for the first time will find to hand a theory which reflects the earlier investigators' opinions as to

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<sup>18</sup> G. Myrdal: *An American Dilemma* (Appendix 2).

what policies were worth examining and what objectives were worth pursuing. . . . But he will not find theories exposing the method by which one might achieve objectives that have never entered the heads of earlier investigators or which they have dismissed as not worthy of consideration" (p. 216).

He argues, however, that preoccupation with particular objectives to the exclusion of others has led to certain defects in existing theory; many important problems have gone unexamined. "The economist must be on his guard, not only against the bias which impels him to the wrong conclusions from a given set of premises, but also against the bias which impels him to work out certain analyses and to ignore others which do not bear on his own interests. Economic theory can become a theory of equal value to all, irrespective of the ends they have in view, only if economists are prepared to undertake a sympathetic study of those conceivable objectives which have not affected the development of economic theory in the past" (p. 220).

This, then, is an attempt to show why economists *ought* to undertake sympathetic study of objectives they have hitherto ignored.<sup>19</sup> The arguments carry little weight. Of course, the economist could not take the whole field of economic phenomena for his kingdom; he is bound to separate out certain problems, and his solutions are none the less true because they are not solutions to all problems. It is difficult to see, also, how the economist could do other than follow his own interests (whether they are practical or theoretical) and neglect what does not interest him. 'Bias' is normally associated with falsification or distortion; if a theorist is biased because he concentrates on some problems and ignores others, then no one could escape it. Walker, in short, is trying to amalgamate two distinct things; in arguing that it is wrong for the

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<sup>19</sup> The tenour of Walker's discussion shows that he does not seriously mean that economists should study *all* conceivable objectives. What he is really concerned about is that economists should not fail to study sympathetically the social objectives characteristic, e.g., of social-democratic governments. But he wants to represent the obligation as arising somehow out of the necessities of economic theory itself.

economist to devote himself to the service of some socially demanded objectives and to ignore others, he tries to support this *moral* contention (for which he offers no ethical arguments) by introducing the theoretical conception of bias (or falsification) though clearly no such thing need be involved.

He is wrong also in suggesting that economic theory cannot be equally valuable to all unless economists "sympathetically study" all conceivable objectives. Whatever the special "end" the economist is trying to elucidate and advance, he may, in doing so, make discoveries of equal value to all social interests, by bringing to light economic laws that any social movement would have to take account of.<sup>20</sup> It is certain that much of existing economic and political theory, even though originally worked out under the influence of "bourgeois" "ends" or ideologies, embodies social laws which anyone who is putting a policy into operation would need to know.

There is really an attempt "to have things both ways" in Walker's handling of the questions. Social theory, including economics, has been deeply influenced by prevailing "values" or "ends" precisely because thinkers, consciously or unconsciously, accepted these "ends", often believing them to be of high moral importance. Walker sees that the influence of these "values" has often led to theoretical shortcomings including the neglect of big problems. He concludes that economists should therefore give equal consideration to all objectives. This will scarcely do. "Values" have been influential historically because thinkers have believed in them; if there is any force in this contention, why should we then expect theorists suddenly to begin devoting themselves to "ends" and "values" which they do not share?

Thus, confronted with Walker's demand, a theorist might well ask why he should give sympathetic attention to objectives which he does not share. Many prevailing objectives will seem to him to be unimportant, either because they do

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<sup>20</sup> This point is made, in reference to Marxist notions of "class science", by Max Eastman in *The Last Stand of Dialectical Materialism* (New York, 1934).

not raise any novel or interesting theoretical issues or even because they do not appear to him to be of any moral significance. (Thus, the "problem" of personal inequalities in the distribution of incomes, for having neglected which Walker reproaches economists, did not seem to economists of the last century to be of such moral importance as it does to most contemporary moralists and economists.) It is clear, then, that Walker's own treatment merely amounts to his urging upon economists a particular ideology. Why should economists accept it? There is nothing in the nature of economics as such which requires the adoption of any ideology, including the ideology of equal service to all ideologies.

The economist's accepting certain "values" and objectives is distinct from his theorising as an economist. "Values" and ideologies have an independent source; they impose themselves upon the economic theorist. Different ideologies and objectives will influence different economists according to their social circumstances. It could not be shown that there is any ideology which is proper to economists as such.

It seems to me, therefore, that instead of trying to have the best of both worlds, the conclusion that Walker should rather draw is that it is not the function of economic science itself to devote itself to the service of social objectives. No doubt, there will always be an abundant supply of politician-economists (these remarks apply to all social scientists); men who will become attached to some political movement or policy and who will be specially interested to apply the findings of economics to the special problems which arise for that movement. But Walker's own criticism of the shortcomings of earlier economists seems to show that the science will develop most freely to the extent that the theorist learns to suspect, and to emancipate himself from, the overriding influence of "objectives" and ideologies, and to concentrate on the issues which are essential to the progress of theory.



## REVIEW.

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OUR KNOWLEDGE OF UNIVERSALS. By R. I. Aaron. Annual Philosophical Lecture, Henriette Hertz Trust, British Academy, 1945. From The Proceedings of the British Academy, Volume XXXI. London: Humphrey Milford. Pp. 28. Price: 2s. 6d. net.

Aaron regards the problem of universals as "exceedingly complex", and says that the multiplicity of different senses in which the term *universal* has been used refer to different aspects of this complex problem revealed to philosophers "looking at this complexity from different angles". The multiplicity, he says, could not have been avoided (cf. "We are not in a position to say of any of these senses that it is the true sense and all the others are false"); but the confusion could have been avoided if philosophers had made clear the sense in which they were using the term (p. 3). Concerning the method of his lecture, he says (p. 4): "I shall try to take into account all of the various usages of the term—or, at least, all of the more prominent usages—and ask of each how we come to know the universal in that sense. . . . I shall so far as possible avoid the metaphysical issues. . . . My approach will be psychological and phenomenological; that is to say, I shall not consider questions of ontology. So far as possible, also, I shall avoid logical issues, though these are not so easily avoided." Because of these principles, the lecture proceeds with a halting step and in many places becomes equivocal and evasive. It may be necessary to begin with linguistic usages but, having tied his own hands, Aaron makes no attempt to discover whether these usages imply factual contradictions; and consequently many of his conclusions can be regarded only as demanding our agreement that words have been used in a certain way.

Thus he says (p. 27), summarising the results of his speculations: "It ought to help us to know that the term *universal*

is used in many senses, that sometimes we mean discovered and sometimes constructed universals, sometimes dispositional and sometimes explicitly conceived universals. We need to acknowledge also, before grappling with the larger problems, the existence of vague universals as well as precise, and universals of things as well as universals of quality and relation." Here we might agree both that our knowledge of anything involves discovery, construction, dispositions and degrees of precision or vagueness and that the term *universal* has been used in all the senses given. But we might disagree that the term *universal* can be taken to have all these meanings except on a subjectivist position, and then only at the cost of logical inconsistency; and these are just the sorts of issue discussion of which is precluded by the principles laid down at the beginning of the lecture. In the same way, consideration of the main question—whether or no it is misleading to speak of "our knowledge of universals" in view of the strong objections that have been directed against the doctrine of abstract ideas and the doctrine of "floating ideas"—is ruled out despite the fact that, if these objections are sound, whatever we know contains "thinghood" as well as universality and there is no such process as "our knowledge of universals".

Aaron, however, is unable to maintain his psychological and phenomenological approach. He cannot avoid giving an objective or logical account of the universal, the feature which he emphasises being recurrence in space. I apprehend the relation *larger than*, he says, "as recurring, that is, as universal": "Do not things also recur? Are there or are there not 'universals of things'?" (p. 8; cf. p. 5). The weakness of this view is that awareness of recurrence presupposes awareness of occurrence, in which universality is already present, so that recurrence cannot be taken as explaining universality. For example, when we say, "X is red", red's being a universal does not depend on anything else's being red; and the point comes out even more clearly in assertions like "X is the Prime Minister of Great Britain", where there is no possibility of spatial recurrence.

Similarly, Aaron's remarks on the extent to which Russell is a nominalist and on the possibility of a "thing-universal" do raise and make some attempt at settling important philosophical questions, but only, again, because he departs from the principles he has set down. As regards the "thing-universal", he rightly rejects what might be called "analyticism", the notion that a "thing" is not itself a universal but a complex of other universals of quality and relation. But his reason for rejecting analyticism is that the notion of substance or thinghood is not contained in the resultant complex of universals, when what is rather the case is that you cannot know anything apart from substance; and the real difficulty of treating terms like *man* as in themselves things which only become universals by analysis is to decide just what is to be included in the ultimate analysis, to show in what respect the contents of the analysis are superior to the term analysed.

To find confirmation of the view that there are universals of things Aaron goes back to a primitive level of experience, at which we "miss the unique individual" and experience the "vague recurring 'whole', the universal". The "whole" is likened to a Gestalt, and it is exemplified in children calling "all the men they see 'Dad'." "They begin with the universal and only later distinguish the particular" (p. 15). In view of the objections already noted against the assumption that we "know universals" it would be more accurate to say that children begin by recognising that certain particulars are alike and only later recognise the respects in which they are unlike; and in this connexion we might mention Aaron's emphasis on the "mind's natural movement in thinking" from generic to specific: "'tree'", for example, "is used by us significantly before we begin to distinguish between the oak, the sycamore, and the beech" (p. 23). Aaron makes this the basis for the criticism of the traditional formal logic; but he provides no evidence for the exclusive naturalness of the process in question. Indeed, there is no such evidence. The word "tree" is also used significantly before we begin to subsume trees, grass, etc. under plants; there is, then, a corresponding move-

ment from specific to general, and neither process has an exclusive claim to naturalness while both processes are formalised in the traditional formal logic.

The important point is, however, that the notion of "wholeness" might be relevant to the question, What is the meaning of the word "man"?, in such a way that when the child formulates an explicit belief, e.g., Men are upright, he need not be taken either as believing the word "man" means upright (in which case there is the above-mentioned difficulty of deciding what is to be included in the meaning) or as believing upright etc. are upright (in which case he would be "believing" a tautology). Aaron does not fully avail himself of the notion of "wholeness", as may be seen from his remark (p. 18), "When as adults we use such words as *man*, *table*, *bed*, we are not then using them in this primitive manner", but presumably as what he calls "constructed universals", that is, as syntheses of elements found, on analysis, to be common to certain particulars. Here, of course, he would be faced with one or both of the difficulties just referred to.

It should be clear, then, that Aaron, fortunately for the value of his lecture, does not keep to his psychological and phenomenological approach. Where he tries to proceed in a philosophically negative manner he merely succeeds in temporarily covering up the issues. He cannot consistently do this because, in the first place, he has a relevant philosophical position of his own and, in the second place, the subject-matter he is dealing with is largely philosophical in character.

T. A. ROSE.

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## NOTES AND NEWS.

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Owing partly to editorial and partly to printing difficulties the Journal for 1945 appears very late, and a whole Volume (reduced in size) is covered by this single issue. These difficulties having now been largely overcome, the appearance of issues for 1946 should not be long delayed, and the size of the Volume will be that of 1944 and the preceding year.

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### ANNUAL CONGRESS.

THE Annual Congress of the Australasian Association of Psychology and Philosophy will be held in Melbourne University from 17th to 20th August, 1946. The provisional arrangements are as follows:

#### Saturday, 17th August:

Afternoon—Address by Mr. M. Palmer (Melbourne) on  
"Methodology in the Sciences".

Evening—Annual Meeting of the Association at 7.30 p.m.;  
to be followed by an Address by Dr. McElwain  
(Melbourne).

#### Sunday, 18th August:

Afternoon and Evening—Social Programme.

#### Monday, 19th August:

Morning—Address to be arranged.

Afternoon—Address by Dr. Davey (Adelaide).

Evening—Address by Professor John Anderson (Sydney)  
on "The Philosophy of Alexander".

#### Tuesday, 20th August (meetings of the Association in conjunction with the Australian branch of the British Psychological Society):

Morning—Address by Mr. J. A. Passmore (Sydney).

Afternoon—Address by Professor W. M. O'Neil (Sydney).

Further details may be obtained from the Hon. Secretary of the Victorian Branch, Mr. A. W. Meadows, c.o. Children's Court Clinic, Carlow House, 289 Flinders Lane, Melbourne, C1.

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A SPECIAL residential Conference on Methodology will be held at the W.E.A. Summer School, Newport, N.S.W., from 31st January to 7th February, 1947. Accommodation is limited, and those members who are desirous of attending should notify the Secretary of the Association, Mr. J. L. Mackie, Department of Philosophy, Sydney University, as soon as possible. The cost for the week will be £2 10s. for each person.

THE following are the office-bearers of the Victorian Branch of the Association for 1946:

*President:* Dr. McElwain.

*Vice-Presidents:* Professor A. Boyce Gibson, Mr. D. Taylor, Dr. K. Cunningham, Judge Foster, Dr. Adey.

*Hon. Secretary:* Mr. A. W. Meadows, Children's Court Clinic, 289 Flinders Lane, Melbourne, C1.

*Committee:* Rev. Father Ryan, Rev. Principal McLeish, Mr. K. Baier, Mrs. A. Bell, Dr. A. Phillips, Mr. A. E. Houston.

The programme for 1946 includes lectures by Judge Foster on "Criminal Responsibility", by Professor A. C. Garnett on "Philosophical Currents in U.S.A.", and by Mr. Hunt on "Cicero".

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THE Sydney Local Branch of the Association has resumed its activities, and several meetings will be held in 1946. Inquiries may be directed to the Secretary, Mr. A. J. Baker, Department of Philosophy, The University, Sydney.

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WE have received the complete illustrated catalogue of Medical, Surgical, Nursing, Dental and Scientific books published by E. and S. Livingstone Ltd., 16-17 Teviot Place, Edinburgh 1. Messrs. Livingstone are prepared to send copies of this catalogue to any address on application.

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